DÆDALUS

HISTORICAL POPULATION STUDIES

J. A. Banks
Bernard H. Slicher van Bath
Jean-Noël Biraben
Paul Demeny
D. V. Glass
Pierre Goubert
Susan B. Hanley
David M. Heer
Louis Henry
T. H. Hollingsworth
Gerald Holton
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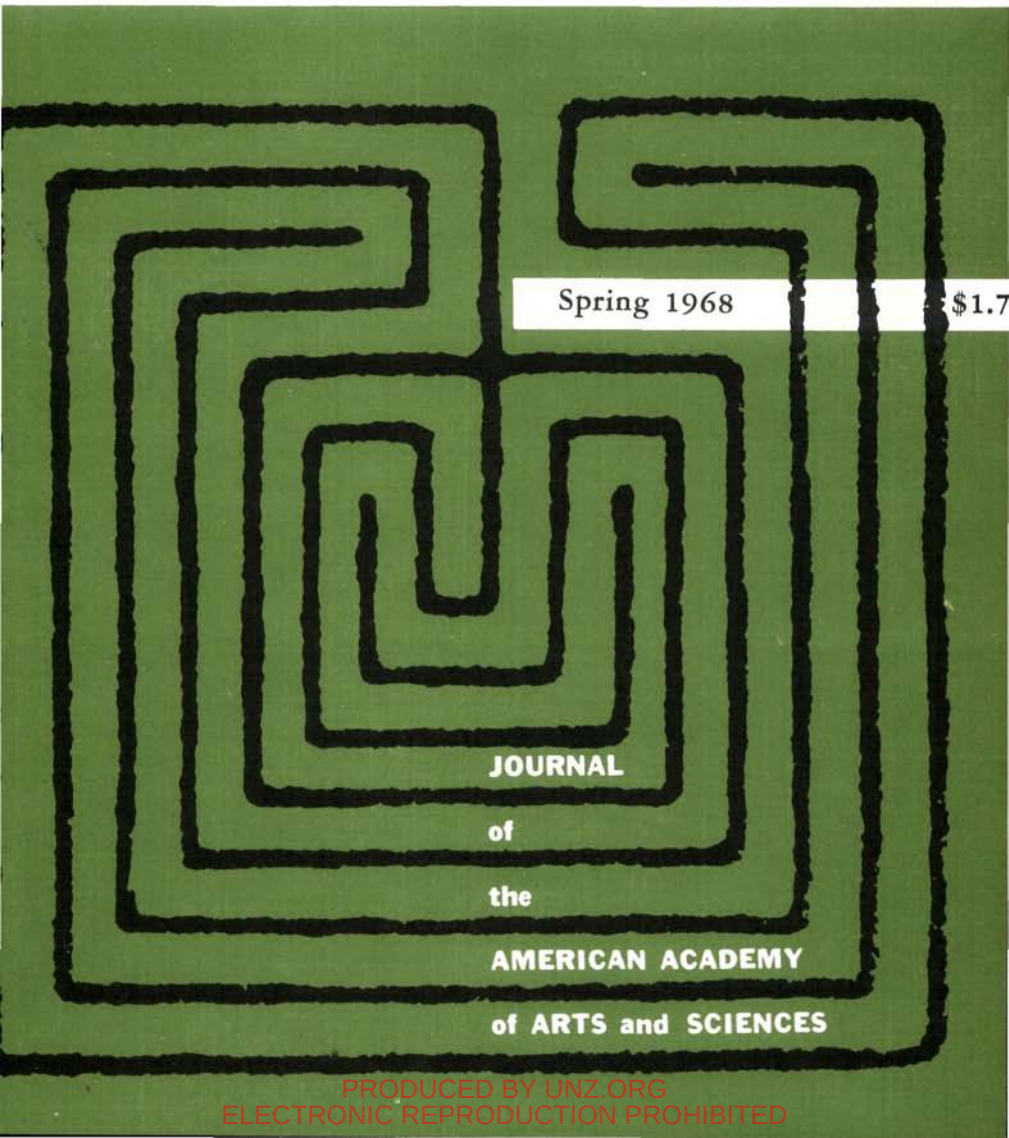
Massimo Livi-Bacci
John T. Noonan, Jr.
Roger Revelle
Joseph J. Spengler
Etienne van de Walle
E. A. Wrigley



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DÆDALUS

Journal of the American Academy of Arts and Science

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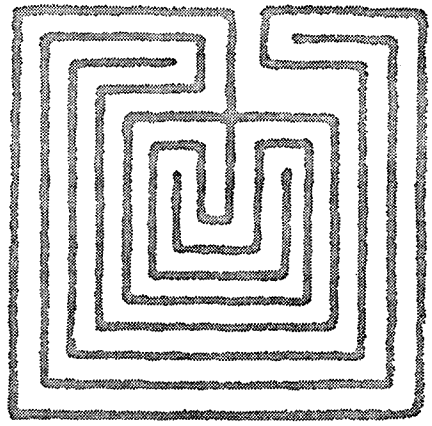

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Preface to the Issue "Historical Population Studies"

STUDIES IN historical demography have expanded substantially during the last decade. A new appreciation of the possibilities of discovery with large implications for both social and economic history has led a growing number of scholars to examine data of a kind previously ignored. While important investigation is being pursued by individuals working independently, there has been an increasing recognition of the advantages of collaborative research for such inquiries. We are now beginning to see the results of these organized efforts.

Research in this area is painstaking and slow; the computer, although an invaluable aid making possible calculations which could not have been attempted before, does not substitute for the deliberate poring over of parish and other records, which is the prerequisite to all new study. It will take time, perhaps several decades, to achieve what is now only vaguely promised—a large-scale reappraisal of social and economic change in Europe and America (perhaps, also, in Asia) over the last four centuries. That one can even imagine this to be a possibility is evidence of the promise contained in these new researches.

This issue of *Dædalus* owes a great deal to Professor Roger Revelle, director of the Harvard Center for Population Studies, who has written the introductory essay, chaired our meetings, and encouraged the Editors to explore a possibility which depended on the cooperation of so many institutions. His enthusiasm led us to embark on an inquiry which we might not otherwise have attempted.

The issue is the tangible outcome of two international conferences on historical population studies sponsored jointly by the Harvard Center for Population Studies and the American Academy of Arts and Sciences. The first of these was held at the House of the Academy in October, 1966; the second at the Villa Serbelloni in Bellagio, Italy, in July, 1967. We are indebted to the Rockefeller

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Foundation for making the Villa, an ideal site for international meetings, available to us.

All but one of the contributors to this volume attended one or both meetings. Other participants included: Professors Bernard Bailyn, Carl Bridenbaugh, Ansley J. Coale, Alexander Gerschenkron, John Habakkuk, John Hajnal, Harvey Leibenstein, Ralph B. Potter, Jr., Henry Rosovsky, and Gustaf Utterström; also, Dr. Olive Banks and Dr. Goran Ohlin. We are grateful to all of them for provocative and instructive discussion.

The Ford Foundation assisted the Harvard Center for Population Studies with funds for these meetings. A grant from the Ford Foundation to the American Academy of Arts and Sciences to encourage interdisciplinary studies has also been drawn upon. We are pleased that the first use of funds from this grant should have supported a project of this kind.

A special section in the back of the issue, *Anticipations*, contains an article by Professor Gerald Holton, entitled "Mach, Einstein, and the Search for Reality." This is intended to introduce a theme which the Academy proposes to explore more fully as it embarks on a study of the intellectual biography of scholars in the twentieth century. This is another undertaking which the Ford Foundation grant to the Academy makes possible.

S. R. G.

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ROGER REVELLE

Introduction to the Issue "Historical Population Studies"

ALMOST EVERYBODY, including most demographers, believes that his great-grandparents had all the children they were capable of having—after they were married, of course. Today, as we all know, most couples in Europe and the United States have about the number of children they want. In the bad old days, a good many of the children died; today, nearly all of them grow up.

Demographers have erected a theory to fit these beliefs and facts: the theory of the demographic transition. In the past, according to this theory, human populations maintained themselves or slowly expanded under conditions of high mortality balanced against high, essentially uncontrolled fertility. During the Industrial Revolution, fertility remained high and uncontrolled for a while, and the average length of life increased. As a result, populations grew rapidly in the Western world, at rates higher than ever before experienced. During the nineteenth and early-twentieth centuries, birth rates began to come down, first in France and the United States and then in the other industrialized countries, due to the deliberate control of births by individual couples. This decline in fertility eventually slowed down population growth (though, on the average, populations in these countries are still growing at rates that were unprecedented on an international scale before the nineteenth century).

To date, the demographic transition from high to low fertility and mortality seems to be chiefly a European, American, and Japanese phenomenon. Only part of it—the decline in mortality—has occurred in the poor countries of Asia, Latin America, and Africa, where most of the world's people live. Because their death rates have drastically fallen, while their birth rates have hardly changed at all, the populations of these countries, which grew relatively

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slowly until a few decades ago, are now expanding more rapidly than the populations of most of the presently developed countries ever did; and no man knows what tragic consequences lie ahead.

Except for their low mortalities, the less developed countries are much more like Japan and the countries of Europe two hundred years ago than those countries today. Under these circumstances, it seems urgent to examine the historical conditions and processes that existed before the demographic transition and during the early stages of the decline of fertility—in the hope of learning something that may be applicable to the enormous problem of reducing birth rates in the poor countries. The articles in this issue of *Dædalus* illustrate work in progress toward that end.

This is not to say that the authors undertook their research with any such practical objective in mind. They were impelled, instead, by historical curiosity, the opportunity to develop and use new research methods, and the desire to understand the conditions of life for ordinary men and women in past times—men and women who left few records beyond the austere annals of their marriages, births, and deaths.

Perhaps the most striking outcome of these studies is evidence that many married couples in some, but not all, of the rural societies of pre-industrial Europe, long before the start of the demographic transition, deliberately limited the number of their children; this was in addition to such "social" methods of fertility control as prolonged lactation and postponement of marriage. (The latter, though highly effective and the only method endorsed by the worthy and innocent Reverend Malthus, was probably not thought of by the couples who practiced it as a device for keeping down the ultimate number of children they would have.)

There were marked differences in levels of marital fertility and, hence, presumably in the degree of fertility control within marriage among the regions of Europe and at different times in the same region. Indeed, the wide range of marital fertility in space and time is one of the principal proofs that some deliberate fertility control existed before the demographic transition. It is hard to avoid Paul Demeny's conclusion that the decline in fertility during the transition resulted not from a qualitative change in reproductive behavior—a "social mutation"—but from quantitative changes in the frequency, dissemination, and techniques of old practices in response to new conditions of life.

A second conclusion comes from these studies. Despite the

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ability and willingness of many couples to limit the size of their families, pre-industrial populations seem often to have grown to larger sizes than they should have attained for a comfortable balance with available resources. Though most people were probably well above the level of bare subsistence, poverty and misery became widespread. Under these "Malthusian" circumstances, the numbers of people ceased to increase, or even declined, probably through a combination of increased infant and child mortality, emigration, social customs limiting fertility, and conscious fertility control. Evidently, our ancestors had to be placed under considerable stress before they would forego the individual benefits of excessive reproduction.

In one respect, Western Europe was *sui generis* when compared to other parts of the world. Everywhere west of a straight line running from Trieste to St. Petersburg, the age of marriage of women was high until recent times, and many women and men never married. This "European marriage pattern," whose importance was first pointed out by John Hajnal, had several significant social consequences—including, for example, the existence of large households containing servants and unmarried relatives, as well as a married couple and their children. From the demographic point of view, however, its most important consequence was a marked lowering of fertility. As a fertility-control device, the European marriage pattern had the advantage of being quickly responsive to changes in economic conditions. When a man could not find a way to support a family, or farm lands were not available, marriages were postponed. Under conditions of prosperity or reduced population pressure—for example, after an epidemic which had killed many of the older people—the number of marriages increased.

In various parts of Europe, significant long-term changes in age at marriage can be shown. In 1626 in Amsterdam, according to Bernard H. Slicher van Bath, 52 per cent of brides were under twenty-five years of age; this proportion was only 32 per cent a century and a half later. By 1850, the average age of women at first marriage in the Netherlands and Belgium was between twenty-eight and twenty-nine years, and about the same for Switzerland. From 15 to 20 per cent of women were still single at age fifty. As Etienne van de Walle shows in his article in this issue, the average age at which women married declined at about the same time as the extent of fertility control within marriage increased, though it is not clear whether or how the two events were causally related.

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In most of the world, women have always married at a much younger age than they have in Western Europe, and nearly all women marry. From the standpoint of understanding possible fertility changes in the less-developed countries, as Paul Demeny points out, a study of the demographic history of Eastern Europe, where young marriages are nearly universal, is perhaps more relevant than a similar study of the West. Moreover, the Eastern European countries remained largely rural until very recent times, and, particularly in Hungary, there was great ethnic, religious, and cultural diversity. This diversity was reflected in wide differences in fertility among provinces and even between nearby villages. The modern decline in fertility began rather late in Hungary, although its average birth rate is now among the lowest in the world, probably below the level needed to maintain the population. In many Hungarian villages, however, the number of living children was already extremely small in the middle of the nineteenth century. Apparently in an attempt to maintain and increase the size of farms, numerous families succeeded in having only one child. The means used for fertility control are not entirely known, but they undoubtedly included the age-old practices of abortion, infanticide, *coitus interruptus,* and abstinence, as well as various "folk" contraceptives.

Because of its lack of industrialization, rural character, and strongly religious society, Spain is also of special interest in considering the problems of the less-developed countries. Making use of all the rather scanty data he could find, Massimo Livi-Bacci has been able to compute life expectancies and fertility rates for Spain from 1768 to 1910. Although as late as 1900 life expectancy was less than thirty-five years in Spain (much lower than that in either India or Pakistan today), a steady decline in marital fertility began toward the end of the eighteenth century and continued through the nineteenth, while the proportion of married females of reproductive age increased throughout the nineteenth century. Spanish fertility was among the lowest in Europe in 1860. There seems little doubt that there was appreciable and increasing deliberate fertility control among married couples. Livi-Bacci suggests that growing social mobility was a significant factor in this pattern of change.

Until the last century, human lives in Europe were cut off not only by a high level of average mortality, but also by intermittent catastrophes—widespread famines and deadly epidemics of plague, smallpox, typhus, yellow fever, and, later, cholera. The most dread-

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ful of these was the plague, which first appeared in Europe in the fourteenth century and recurred at intervals until the eighteenth. Jean-Noël Biraben describes the last important plague epidemic in Western Europe. This began in Marseilles in 1720, following the arrival of a plague-infested ship from Syria, and ravaged Provence and Languedoc for more than two years, killing over a hundred thousand persons. Nearly half of the inhabitants of Marseilles, Toulon, and Arles died. The proportions killed were even higher in some smaller towns, but a large number of settlements escaped with relatively few deaths. The epidemic spread along highways and attacked all cities of ten thousand or more inhabitants in the affected region of southern France, as well as many small towns. It was particularly virulent during the summer; it ended suddenly in October of 1722.

Fairly reliable information on catastrophic mortalities from epidemics and "subsistence crises" (a demographic euphemism for famine) is relatively easy to obtain, for example, from parish registers, which record the month-by-month numbers of burials. For times prior to the advent of modern censuses and vital records, however, it is much harder to estimate the average levels of mortality or age-specific mortality rates—especially infant and child mortalities. Using the powerful tool of family reconstitution from parish registers developed by Louis Henry and his collaborators, E. A. Wrigley has been able to compute the changing levels of infant mortality and life expectancy in the Devonshire village of Colyton over three centuries. Of every thousand children born in Colyton during the reign of Elizabeth I, between 120 and 140 died before the age of one. The numbers of infant deaths increased somewhat during the seventeenth century. There were between 160 and 200 such deaths per thousand live births during the first half of the eighteenth century (compared with about 20 in England today and 160 in India), but by the beginning of the nineteenth, this proportion fell back to the Tudor levels. Corresponding to the high levels of infant and child mortality, the expectation of life at birth before 1600 was between forty-one and forty-six years; from 1625 to 1699, it was between thirty-five and thirty-nine years; and in the middle of the eighteenth century, between thirty-eight and forty-five years. Today, life expectancies are about seventy years in England and about fifty years in India.

As Wrigley points out, there is considerable evidence from burial and baptismal records in other parishes that the increased

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mortality found in Colyton in the seventeenth century occurred throughout England. The causes are obscure; they may have been related not to a lowering of living standards, but to the presence of new infectious diseases brought from other continents during the worldwide expansion of European travel and conquest. It seems apparent from the relative constancy of numbers of births and the steady rise in number of marriages found by Wrigley for Colyton throughout the eighteenth century that there was a considerable degree of deliberate control of fertility within marriage, at least during the latter half of the century.

In the early part of the eighteenth century, deaths exceeded births in Colyton, whereas in London in 1696-98, D. V. Glass finds that baptisms and burials were about equal. At least temporarily, the city may have ceased to be, as it had been for thousands of years, "a consumer of men." Glass's study gives us a glimpse of pre-industrial urban ecology: The middle classes lived in the center, the poor on the outskirts of the city—just the reverse of the situation in United States cities today. Nearly two thirds of the inhabitants were unmarried, and 55 per cent were under twenty-five years of age, reflecting both the prevalent Western European marriage pattern of the time and the high fertility of the people (compared with 44 per cent never married and 37 per cent under twenty-five in England today). Mobility was also high, and the city was apparently growing rapidly, with perhaps as many as 13 per cent of the inhabitants leaving within a year and as many as 27 per cent coming in.

Except for the differences between country and city, not enough research has been done to indicate the degree of demographic homogeneity or heterogeneity in pre-industrial England. Through the work of Pierre Goubert and his students and colleagues, however, it is clear that there were marked demographic differences among the different regions of France. Using the methods of family reconstitution from parish registers employed by Henry and Wrigley, Goubert shows that the levels of infant mortality and the fertility of married women were high throughout the eighteenth century in Brittany, a province distinguished by its "curious economy," Catholic fervor, and general backwardness. A woman marrying at age twenty would, on the average, have ten or eleven children, half of whom would die before they were ten years old. Rapid population growth was prevented by late marriage. The average age of marriage for women was always more than twenty-five years, and

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10 to 15 per cent of the women in Brittany never married. According to Goubert, there was not the slightest trace of birth control within marriage in Brittany, nor were there many illegitimate children, in contrast to England. "Virtue is French" in the eighteenth century. Birth rates and child mortalities were markedly lower in the southwest of France and in Normandy than in Brittany. The intervals between births were thirty months or more in the southwest, instead of twenty months, as in Brittany. This lower fertility was counterbalanced by the survival of about two thirds of the children past their tenth year. A sporadic and intermittent birth control was practiced, perhaps by the use of *coitus interruptus*, throughout the eighteenth century, at least in the southwest. Systematic birth control certainly developed toward the end of the century in Normandy and the region of Paris.

Views of pre-industrial populations in presently developed countries are given by Bernard H. Slicher van Bath for a period from 1500 to 1795 in the Netherlands and by Susan B. Hanley for Tokugawa Japan (1679-1834). In both countries, the total population about doubled during the early part of the period, and there was a rapid growth of cities. Tokyo was probably, as it is today, the largest city on earth in the eighteenth and early-nineteenth centuries, having more than a million inhabitants. Both Japan and the Netherlands apparently suffered from heavy population pressures during the latter half of the period studied, and the living standards of the poor deteriorated. In Japan, about six persons apparently had to be fed from each cultivated acre (compared with 1.7 persons per acre in India today), and agricultural productivity may have been strained near the limit, even to meet human physiological requirements. In both countries, there were marked regional differences in population growth. These were apparently determined by the extent of economic development and land reclamation. In Japan, for example, there was a marked increase in commerce, textile manufacturing, large-scale commercial agriculture, fisheries, and arable land in the regions of greatest population growth.

Some of the difficulties and pitfalls of historical demography are described by J. A. Banks and T. H. Hollingsworth. Banks warns that we must beware of "the ecological fallacy"—the assumption that associations based on data about sets of persons are valid estimates of the associations that would have been obtained had the data been about individuals. For example, differences in fertility

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among regions, some of which are more urbanized than others, need not necessarily be interpreted as differences between urban and rural fertility. They may be the result of other phenomena for which urbanism is, at most, an intermediate variable. Hollingsworth observes that error, bias, uncertainty, mistakes, and outright lies can all contaminate historical demographic data. The most difficult problem results, however, from failures to record relevant events. In parish registers, for example, many infant births and deaths may have gone unnoted by even the most conscientious clergymen. He points out that a historical demographer should not let himself be inhibited by thinking about data in black and white terms. Few numbers are so good that they can be accepted without question or so bad that they are useless.

The historical determinants of human fertility are discussed by David M. Heer, using a conceptual scheme first proposed by Joseph J. Spengler. The decision of a married couple to have or not to have a child depends on the value they place on an additional child compared to the value of other goals; the cost of an additional child, both in monetary terms and in time and effort, relative to the costs of other goals; and the resources of income, time, and energy available to the couple. Per-capita resources of income and leisure time greatly increased in Europe and the United States over the last hundred and fifty years, while fertility markedly declined. Hence, if Heer's scheme is correct, changes in values and costs must have more than counterbalanced the effects of increased income, with the net result that fertility was depressed. Heer describes in some detail how this may have happened.

In a similar vein, John T. Noonan, Jr., examines the origin, dissemination, acceptance, and effects of the cluster of ideas that underlies Western Christian sexual morality and family responsibility. He believes that these ideas were at least one of the causative factors in the typical pattern of late marriages and widespread celibacy which contributed to the suppression of fertility throughout Western Europe until relatively recent times.

Joseph J. Spengler has written the only article in this issue which discusses the socio-economic consequences of demographic factors. In his view, the suppression of fertility that resulted from the Western European marriage pattern and the consequent low rates of population growth before the nineteenth century allowed the accumulation of savings for investment and cushioned the impact of human numbers on land and other resources. At the same

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time, it augmented per-capita productivity by its effect on the distribution of the population—with relatively low fertility, the proportion of the people in the productive age group of fifteen to fifty-nine is comparatively high.

"Historical demography is in its heroic age," says Wrigley. Exciting and puzzling discoveries are continually being made, and the marginal returns to research effort can be expected to increase for some time. Each new analysis helps to narrow the uncertainties of previous studies, but raises new problems of its own.

The causes of this excitement are clear. As Louis Henry points out, modern historical demography is essentially a new science. It received its greatest impetus after World War II, when longitudinal studies of the ties between successive generations began to take the place of cross-sectional studies, and powerful new techniques were developed for analyzing a variety of historical population data. More fundamentally, demographers realized that human populations are inscribed in time and can be understood best when they have been traced back into the past as far as usable observations permit.

To be sure, such retrospective analyses alone may not be enough for understanding. One must also use what Banks calls the "hypothetical deductive method" of sociology—the formulation and testing of hypotheses about a social phenomenon against a body of known facts, including especially the social environment in which the phenomenon occurred. Banks illustrates his point by stating three hypotheses about the European marriage pattern in the sixteenth to nineteenth centuries, in terms of the future time perspectives of persons who postponed marriage and the possible shift over the centuries in their attitudes from Christian asceticism to a more worldly, prudential view. He is convinced that the most trustworthy sociological results will be obtained when the historical or documentary approach and the sociologist's method of surveys of living persons can be used to confirm and reinforce each other.

Henry concludes that historical demography, as it has evolved during the last twenty years, is not simply a marginal part of demography. It is demography itself, just as demography, being a study of human populations in time, is history itself. David Landes might not disagree, but he points out that his fellow historians will be convinced only when they can see that demographic studies foster a greater understanding of underlying historical processes. The new science of historical demography has devoted almost all

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its efforts to the determinants of population change and very few to an examination of its consequences. We do not understand the effects of population growth or stagnation on the course of economic and social development; the consequences of greater longevity on the allocation of resources and the distribution of political power; or the effects of differential fertility and mortality on the pathology of cities. These questions cannot be answered solely by the present techniques of demographic research. Extra-demographic variables must be introduced, some of which cannot be quantified. This will require a joint effort, combining the intellectual resources of historical demography with those not only of other historians, but of sociologists, economists, and specialists in resource analysis.

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DAVID LANDES

The Treatment of Population in History Textbooks

In principle, the historian is supposed to learn and know whatever is necessary for him to understand and explain his subject. In fact, the wider the range of his concerns, the more he falls short of this ideal. No one is supposed to know so much as the writer of the general textbook, intended to cover all aspects of the history of a country, or Europe, or the West, or the world over a given period; yet all professional historians, to say nothing of students, know enough not to expect anything like omniscience from these panacean compendia. Indeed, a cynic might argue that a good teacher wants a text with just enough shortcomings to enable him to shine.

The weaknesses of general history were less apparent when history itself was narrowly confined in its interests; when the story was limited to the doings of "important" people—kings, courtiers, politicians, generals. As the range of interests within the discipline has widened to include the mass of common folk as well as the select few and to comprise the full array of human phenomena—economic, intellectual, social, psychological, as well as political, diplomatic, and military (to embrace the realm of everyday, banal existence as well as that of the unique, eventful exercise of power)—the gap between ideal and performance has increased. This is particularly true of those aspects of the subject that have become the province of highly-trained specialists using esoteric techniques. Economic history is the most obvious example. The history of population is another.

The study of population is not so old as history (that is, history as a defined field of knowledge), but it does go back a long way, to Graunt and Petty in seventeenth-century England, to the Abbé Expilly in France a century later, to Süssmilch in the Prussia of Frederick the Great. These fathers of modern demography began

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with the concern to establish the facts of population change, but they and their successors rapidly moved on to the analysis and explanation of change. By the late-eighteenth century, demography had assumed the shape that has prevailed to this day—that of a discipline combining increasingly sophisticated techniques of quantification with a core of theory that aims to relate the diverse statistical data.

The development of historical demography as a defined subfield of demography and, at the same time, of history (one of an increasing number of such dual specialties) is much more recent.[1] Some of the first manifestations of interest in the subject come in late-nineteenth-century France, where demographers and historians, as well as politicians and statesmen, were exercised about the low and declining birth rate, which had obvious implications for France's status as a European power. The problem was that not only was French population almost standing still, but the number of Germans was growing rapidly, so that demography threatened to put the seal on the defeat of 1870. As a result, there was a spate of studies on the character and causes of France's demographic stagnation, most of them pessimistic and some of them frankly alarmist. In 1897, Jacques Bertillon, one of the foremost "natalists" of his day, gave an anguished warning to his compatriots: "In fourteen years Germany will have twice as many conscripts as we. Then that people that hates us will devour us! The Germans say it, write it, and will do it!"[2] It was in this context that Levasseur wrote his sober three-volume history *La population française* (1889-92). It remains a useful reference work to this day.

At the same time, German historians were also beginning to turn their attention to population, though less to the German population of their own day, which was obviously developing satisfactorily, or of the immediate past than to that of other countries or of far earlier times. Ignaz Jastrow did a study of the population of German cities at the end of the Middle Ages and in the early modern period (1886); Karl Bücher published his classic monograph on the population of late medieval Frankfurt (1886); Julius Beloch brought out a series of monographs on the population of the Greco-Roman world (1886, 1899, 1903), of Italy from the sixteenth to the eighteenth century (1888), and of Europe in the Middle Ages and Renaissance (1900); and Karl Lamprecht devoted a substantial chapter of his economic history of medieval Germany to demographic issues (1885).[3]

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In this area the British, who were the pioneers of demography as a contemporary science and were always among the leaders in population theory, were laggards. Not until G. Talbot Griffith brought out in 1926 his *Population Problems of the Age of Malthus* did the British historian find a theme that aroused his curiosity and intellectual passion; and from that day to this, the subject of population growth during the period of the Industrial Revolution has remained the central problem of British demographic history.[4] Contemporaneous with Griffith or following after, there have come Margaret Buer, T. H. Marshall, Barbara Hammond, Dorothy George, Thomas McKeown and R. G. Brown, J. D. Chambers, David Glass, T. H. Hollingsworth, K. H. Connell, H. J. Habakkuk, J. T. Krause, W. A. Cole, P. E. Razzell, and others—all of them adding fuel to the controversy.[5] Any such list is necessarily incomplete not only because space limitations prevent a complete enumeration, but because new names are added all the time.

This preoccupation with population change during the course of industrialization is, in part, dictated by the nature of the evidence: With discrete exceptions, continuous, wide-ranging demographic statistics begin in the late-seventeenth and eighteenth centuries. Intellectual considerations, however, have been paramount, specifically the conviction that the so-called demographic transition from a Malthusian equilibrium of high death rate/high birth rate to a new equilibrium of low death rate/low birth rate is one of the central phenomena of modern history—indeed, that it is a core aspect of the process of modernization itself. Hence, interest in Britain and elsewhere has broadened to encompass the other stages of the process—in particular, the adjustment of fertility to lower mortality.[6] This concern has received new impetus from the general anxiety about the population explosion of pre-industrial areas in the twentieth century. Can history, asks the demographer, shed some light on the determinants of population growth—their nature, relationship to one another, susceptibility of management, and the like?

We have, then, a growing body of work in demographic history of an increasingly sophisticated character. The range of inquiry and analysis has widened, and new statistical and research techniques have been introduced by specialists who have come to the field from the demographic, rather than the historical side.[7] Few historical specialties have grown and changed so rapidly, and this change is in turn testimony to the recognized importance of the

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subject. It is all the more interesting, therefore, to consider the place of this new area of inquiry in the larger field of history *tout court*. Specifically, what place has it held in the general textbooks? And through these, what information about the history of population has been transmitted to the younger generation? An effort, however cursory, to answer these questions will tell us something about historians as scholars and about the quality of instruction in the discipline. It will also suggest some thoughts about the relevance of the concerns of the demographic historian (or historical demographer) to those of general history.

For the purpose of this inquiry, I have confined my searches largely to books in English on European or Western history, with special attention to the textbooks in use in American colleges and universities. Nowhere else, in the Western world at least, does the standard, required textbook hold so important a place in higher education, and nowhere else are the rewards to the author of a successful text greater; hence the readiness of even the best scholarly talent to devote itself to this kind of work. The textbook is, thus, a reasonably accurate reflection of current historical scholarship. (I say "reasonably accurate" because these manuals have obvious built-in constraints that necessarily distort the author's message. They have to be simple, and history is usually complex. They should be as free as possible of jargon and technical matter, and it is hard to treat demography without some recourse to numbers and abstruse theoretical concepts. Finally, in order to make money, they ought to be lively; but population is not the most exciting subject for the average student in a required course in "Western Civ.")

The earliest textbooks and general histories—those of the pre-World War I period—devote little, if any, attention to population. This is not surprising, in view of the general neglect and ignorance of the subject and the narrow view of history that was prevalent. Still, one might have expected more from a collaborative effort like the *Cambridge Modern History,* which brought together experts from many countries and a wide range of specialties. Yet Volume 6 on the eighteenth century, published in 1909, seems to have nothing on population, even in the chapter on Ireland; and it contains no chapter whatever on the economic and social history of the period, where demography might have found a place. By contrast, Volume 11, *The Growth of Nationalities* (1909), does have a chapter by J. H. Clapham, the future professor of economic

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history at Cambridge, on "Great Britain and Free Trade (1824-52)." Clapham makes a brief reference to the continued growth of population (to which the interested reader might have asked, continued from what?), and he treats the famine in Ireland as a precipitating factor in the repeal of the Corn Laws. Yet the chapter comments on the sharp drop in Irish population due to the famine and emigration without mentioning the rapid increase in numbers that preceded the catastrophe; and although it speaks of emigration from the United Kingdom in these years as checking "the gloomy fears of the Malthusians" (p. 20) and easing the labor market, it sheds no light on the source of these fears or this pressure. The last volume in the set, Vol. 12, is *The Latest Age* (1910). Again there is a chapter, by Sidney Webb, dealing with economic and social history. Webb's concern, however, is primarily with social movements, conditions of work, and welfare programs. The only reference to population, almost *en passant,* comes in a brief discussion of urbanization.

If the leading European historians, writing for a constituency that included their fellow scholars, could pass over the subject of population so lightly, one should not expect more of textbooks addressed to a more innocent clientele. Carlton J. H. Hayes brought out his *Political and Social History of Modern Europe* in 1916; the book was destined, in various avatars, to be the most successful American text of the interwar years. It disposed of population and the Industrial Revolution in three sentences (Section 2, p. 76):

> The development of commerce and industry has been attended by remarkable increases of population—for clothes could be made and food produced more easily than ever before in the world's history. In the first half of the nineteenth century, the population of England was doubled. During the nineteenth century the population of Europe, roughly speaking, grew from 175 million to 393 million.

This was more space than Hayes was prepared to devote to the subject some thirty-five years later. In a textbook entitled *Modern Europe to 1870* (1953), which he described as "an essentially new work" in which national politics and policy would receive "chief attention, but not to the neglect of those social, economic, and ideological factors which shape or at least condition political thought and action," he found no room for population at all.

Hayes was a political historian of the traditional variety. What, however, is one to make of the representatives of the so-called New History, who were going to revolutionize the discipline by widening its scope to include those more enduring elements and

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determinants of change—social, economic, cultural—that the conventional political narrative had neglected? One of the two leaders of the movement was James Harvey Robinson, who wrote an *Introduction to the History of Western Europe* (first edition, 1902; new brief edition, 1931). In the 1931 edition he took care of the population explosion in a sentence: "Moreover, the population of Europe nearly doubled during the nineteenth century, owing to the quick and ready facilities for distributing food"—offering no statistics, no reflections on the significance of the change. He did, however, follow this with a paragraph on the rise of cities. Robinson also joined with Charles Beard in writing *The Development of Modern Europe: An Introduction to the Study of Current History* (first edition, 1907; revised edition, 1929); here the subject of population was simply never mentioned.

One could cite numerous other examples of this neglect or swift disposal of the subject in the textbooks of the interwar years—the work of such distinguished scholars as J. Salwyn Schapiro, Edward P. Cheyney, H. A. L. Fisher, Robert Ergang, Franklin Palm.[8] Nevertheless, sins of omission may be less serious than sins of commission. The Thompson-Palm-Van Nostrand text, in an unguarded Malthusian moment, declares that Great Britain, "by her unrestrained expansion of population and facilities for production in certain lines, threw her economy out of gear to such an extent that she was threatened with a shortage of food, clothes, and other necessities of life" (p. 818). The Ferguson and Bruun volume, which tries to give more attention to the subject than most, speaks of the surge of population in the eighteenth century and then offers a graph showing a smooth parabolic increase from 1630 on.[9] The Boak-Hyma-Slosson text devotes a page to the population explosion of the nineteenth century, which it attributes to a rise in the birth rate in Russia, a fall in the death rate in Western and Central Europe.[10]

The palm in this domain, however, must go to Ferdinand Schevill, whose *History of Europe from the Reformation to the Present Day* appeared in 1925. An introductory chapter declares that the book will be representative of the New History—that is, it will be a history of civilization. Although there is no mention of population in the Index, the book does, in fact, devote more space to the subject than any other text of these years that has come to my attention. In a chapter on "The Industrial and Social Revolution of the Nineteenth Century," the author treats the rapid increase in

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numbers as a consequence of the Industrial Revolution (p. 516):

> Wherever the Industrial Revolution has taken root it has greatly increased population for the simple reason that the production of more goods has enabled more people to live. Statistics would seem to show that the increase is only partially due to an improved birth rate (or lowered death rate) and that it most often presents itself to view as migration from the rural districts to the urban centers in possession of the machines. In the case of England, which serves as the industrial type, the total population doubled in the first half of the nineteenth century and has continued to increase since at a scarcely slackened rate.

The discussion continues by noting the increase in food supply thanks to imports from abroad. Schevill then moves on to Germany and offers the following analysis: "The German agricultural population has at best remained stationary, proving that the considerable German increase in population belongs to the towns and is of industrial origin." Finally, he generalizes the remarkable fertility of German cities to Europe as a whole: "Conceding that Europe's amazing growth of population is overwhelmingly industrial and urban, we are obliged, in considering the problem of the maintenance of this human increase, to take account not only of the new foreign markets but also of the improved productivity of the soil."

Of course, if one turns from general histories to specialized textbooks, specifically to those in economic history, the picture is more reassuring. The attention accorded population varies, from the passing references of Lilian Knowles to the more extended discussions of A. P. Usher and Herbert Heaton.[11] Usher's treatment is especially impressive, given its precocity. He wrote before Griffith opened up the whole debate. In the absence of a convenient body of monographic literature, Usher went to the British census of 1841 for Rickman's estimates of population in the eighteenth century, and he hit on the interesting device of contrasting the actual course of population growth (insofar as he could establish it) with the course predicted by Gregory King at the end of the seventeenth century. King expected the population of England and Wales to reach 7.35 million by 1900; by that date the number had reached 32.5 million.[12] Usher has little to offer, however, on the causes of this unanticipated surge, except to suggest that industrialization brought with it a greater international division of labor, and that specialization made possible a larger food supply.[13] "The dismal forebodings of the Malthusians," he wrote, "seem very remote to us, and it is equally difficult for us to appreciate Liebig's feeling that

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the application of scientific theory to agriculture would be of inestimable humanitarian worth by reason of freeing the world from the prospect of indefinitely increasing pressure upon food-supply" (p. 269). Usher's confidence seems as remote to us as the dismal forebodings of the Malthusians.

Heaton, writing in 1936, had the advantage of more than a decade of further demographic research and debate. His treatment, moreover, is the more interesting because it is relatively up to date and reflects the strengths and weaknesses of contemporary work. Thus he states that the population of England and Wales grew substantially (by 23 per cent) from 1700 to 1760, whereas we today are inclined to see it growing at about half that rate; he explains the rise and the fall in the death rate as being due to better sanitation and medical care, whereas we have been inclined to discount this factor, since the work of McKeown and Brown.[14] Heaton sees the increase in the supply of food as a response to, rather than a cause of, population growth.

Heaton devotes more space to population change in the nineteenth and early-twentieth centuries, relying on Marshall for Britain and on R. R. Kuczynski for Europe as a whole.[15] The discussion focuses on the final phase of the demographic transition: the general fall in the death rate from 1870 on and the concomitant drop in fertility, due to later marriage and the spread of birth control. Writing in the middle of a world depression, when birth rates had fallen to their lowest point in recorded history, Heaton offered a gloomy prognosis: "This development must produce stationary and then declining populations in those countries which for four centuries have been the energetic and dominant economic and political regions of Europe" (p. 744). And citing Beveridge: "Such a development, visible already over other parts of the world where the white race has gone, would indeed be 'a turning point in history'" (p. 744). Heaton went on to discuss the economic and social consequences of this change, noting a fall in the demand for food and raw materials, a shift in expenditure toward consumers' durables, luxuries, and services, and an increased concern for the old as against the young. He then concluded with further tentative predictions: "Stability and security will seem more important economically and politically than will opportunities for enterprise and advancement. But those countries which still possess youth and energy, and which want outlets for their teeming populations, may not be willing to let the older ones enjoy either stability or security

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undisturbed" (p. 745). The whole makes interesting reading and leads one to reflect on the hazards of contemporary history.

After World War II, numerous changes were made in the history curriculum of American colleges and universities. Sometimes world history (or "world civilization") replaced European history as part of an effort to live up to the new ideal of One World, or the differentiated history course was dropped in favor of general courses in social science or social studies—sacrificed in the name of disciplinary ecumenism. Whatever the title given to the introductory survey course, the textbook usually had to say something about population, the more so as it was rapidly becoming clear that the non-Western peoples of the world were experiencing the kind of population explosion that the West had known in the eighteenth and nineteenth centuries, but under far less favorable economic conditions. Historians differed on the seriousness of the problem posed by this increase in numbers, but all agreed that population was a central issue for the world citizen of the mid-twentieth century.

In addition, the growing concern with population had its impact on scholarly research. The war had necessarily entailed an interruption of training and writing in this field; work did not stop, but it slowed down. With the return of peace, however, demography picked up again stronger than before, and this revival found expression in new research centers and publications: the Institut National d'Etudes Démographiques in Paris, with its quarterly *Population* (1946 on) and its monograph series; and the British *Population Studies,* which made its appearance in 1947. By providing opportunities for communication and publication, these further encouraged research in the subject, so that demography rapidly became one of the most creative and productive branches of social science. This advance was bound to make itself felt—first in adjacent fields like economic history, eventually in general history.

During the immediate postwar years, most schools made do with the textbooks of the interwar period, some of them hastily revised to meet the needs of a new generation of students. Such revisions, however, generally entailed no change in the treatment (or nontreatment) of population history. They usually consisted in the addition of chapters on the events of preceding years—the war, the peace, postwar reconversion and reconstruction. (These additions have the virtue of bringing a textbook right up to the

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minute, and both teachers and students like this; they also have the advantage for the publisher of making previous editions obsolete, which cuts down the use of secondhand copies.) It was not until the fifties, therefore, that one saw the first fruits of the new concern with population.

One of the most innovative of the new texts, and one of the most successful, was the Brinton, Christopher, and Wolff *History of Civilization* (1955), which went into six printings in the space of two years and has continued to flourish to this day. As a textbook, its great strength lies in its treatment of ideas and cultural achievements; and some might argue that this is what we mean by civilization. The book does not ignore the economic and social aspects of history, but it deals with population in passing. There is a paragraph, for example, on the European population explosion citing figures for Britain and Russia (tables are carefully avoided; they turn some readers off). This is followed by a brief discussion of urbanization and its social consequences: "The most important social change flowing from the industrial revolution was not the increase in the population but the alteration in its structure and balance" (pp. 202 f.). The subject returns later, on a theoretical level in a review of Malthus, which provides the occasion for a reference to the Irish famine and depopulation. One finds in a chapter on India: "It is clear that if the Indian masses are to attain the standards of human welfare that Westernization seems to make people want, India will have to solve—and not by starvation or war—her problem of population. Her citizens will have to interfere with 'nature' and institute birth control, something abhorrent to the basic ways of Hinduism" (p. 629).

The Brinton-Christopher-Wolff text covers a lot of ground, and one can well understand why even an important topic like population can receive only brief attention. What happens when one narrows the scope of the story?

One of the standard history courses in American colleges is the one labeled Europe since 1500, or Modern Europe. In this field, the leading text over the last decade has been Robert Palmer's (later Palmer and Colton) *A History of the Modern World*,[16] which has now seen three editions. In the latest edition, this textbook does better by demography than any other in the field. There is a full discussion of "Basic Demography: The Increase of the Europeans," which is broken down into sections on the rapid growth during the eighteenth and nineteenth centuries, the stabilization

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of the European population, the growth of cities, and the transoceanic migration. The authors are not afraid to use tables or charts (this is exceptional) or to talk of birth and death rates per thousand (numbers, particularly ratios and similar compound numbers, frighten history students). Moreover, they are ready to go at length into the consequences as well as causes of the demographic transition.

How do they account for the great increase in numbers during these centuries? They start with a disclaimer: "The causes of sudden rise in world population after 1650 are not known" (p. 559). They then become more affirmative: "All students agree in attributing the increase to falling death rates rather than to increasing birth rates. Populations grew because more people lived longer, not because more were born." The authors offer five reasons for why people lived longer: the rise of "organized sovereign states," which brought an end to civil war and "chronic violence"; the elimination of certain endemic diseases—the subsiding of plague in the seventeenth century, the "conquest of smallpox" in the eighteenth; the increase in agricultural output, "beginning notably in England about 1750"; better transportation, which put an end to local famines; and the rise of machine industry, which enabled Europeans to trade manufactures for food (pp. 560 f.). Most demographic historians, of course, would not accept the assertion that smallpox was conquered in the eighteenth century, and agrarian specialists would object to the notion that a significant increase in food output did not begin until the mid-eighteenth century.[17]

Palmer and Colton allude to the consequences of this surge of population for European power: "The ascendancy in modern times of European civilization, or roughly of the white races, has been due in some measure to a merely quantitative advantage" (p. 561). But they see its effects primarily in urbanization and migration. The former was of crucial importance, at least as much for psychological and cultural reasons as for social and political: "The great city set the tone of modern society" (p. 563). The migration, they note, involved some sixty million people (perhaps a fifth of whom later returned) and "towers above all other historical migrations in magnitude, and possibly also in significance, for it was the means by which earlier colonial offshoots of Europe were transformed into new Europes alongside the old" (p. 565). Interestingly enough, in the actual analysis of this movement, Palmer and Colton give almost no place to demographic considerations. They note the

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contribution of cheap steam transport, the pull of land and jobs in the New World, and the push of famine or political persecution. Above all, they stress the permissive liberalism of the eighteenth and nineteenth centuries, when governments generally suffered their subjects to pick up and leave, taking their wealth with them. They do nothing with the correlations between rates of population growth and rates and sources of emigration.

The most original feature of the Palmer-Colton treatment of demography (it has no parallel in any of the other textbooks I have examined) is the discussion of the causes and implications of the shift from large to small families beginning in the last decades of the nineteenth century. On the side of causes, the authors stress the "codes of modern society": to begin with, literal codes like the Code Napoléon, which required a roughly equal division of inheritances and hence encouraged those who wanted to preserve their patrimony to limit the number of their children; and then figurative codes, the values of modern society—the preference for leisure and recreation, ambition for the social advancement of children, the effort to lighten the burdens of the wife and mother. The authors note, however, that small families threaten the continuity of society; that "those societies in which individual welfare is most highly prized, by the very means they take to promote it, become smaller than the societies which value mass and collective strength." They add that "what people consider to be welfare easily passes into self-indulgence, and that some of the most civilized persons have few children, or none at all, for the most grossly selfish reasons" (p. 563). (For a textbook, this kind of moralizing is most unusual.) To be sure, the drop in the birth rate was reversed after World War II, but the "permanent effect" of this reversal "is not yet clear" (p. 561).

Another postwar textbook in this field is Paul H. Beik and Lawrence Lafore, *Modern Europe: A History Since 1500* (1955). Neither of the authors is primarily a specialist in economic and social history. Beik has concerned himself with the ideas and institutions of pre-Revolutionary France. Lafore has published on European diplomacy on the eve of World War I; he also writes novels. But the authors are concerned to link social change to other aspects of historical change. In a section on "Society and Politics," population makes its appearance as one of an array of forces changing the European world: "Although the countryside looked much as it had for centuries, there were accumulat-

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ing during this era the people, money, roads, ships, towns, and machines which were to make Europe astonishingly different in the nineteenth century" (p. 193).

The text continues rather timidly: "For one thing, population was growing—no one knows quite why. [Again the precautionary disclaimer.] Statistics are lacking, but good guesses can be made." In fact, the authors proceed to offer data on population increase in Britain, France, Prussia, Spain, and Russia—though the accuracy of these data leaves something to be desired. They do venture an explanation for at least some of this increase: "Europe's increase was part of the great population growth which has come in the world since the eighteenth century. It is a historical fact explainable in some regions by the better order and improved agriculture which kept a greater percentage of the newly born alive, in other areas less easily explainable but none the less real" (p. 194).

This is the most extended discussion of population in Beik and Lafore. The subject returns at intervals—in a chapter on "Revolutionary Economic and Social Changes" of the nineteenth century, in a section on "Changing Social and Economic Problems" of the period before World War I, in chapters devoted to Great Britain, France, and Russia. In the course of these brief allusions, the authors remark on the growth of cities (pp. 411 and 569), the scope of emigration from Europe overseas (p. 568), the changing distribution of occupations, the political implications of the discrepancy between German and French demographic patterns (p. 621), and the impetus given by Russian population growth to the land hunger of the peasantry, "which played so important a part in the twentieth-century revolutions" (p. 682). In sum, the book offers a dispersed, unsystematic, but not indifferent treatment.

If we narrow our focus even further, we come to a text like David Thomson's *Europe Since Waterloo* (second edition, 1965). Thomson is primarily a political historian, but his short study of *Democracy in France: The Third and Fourth Republics* (third edition, 1958) gives considerable space to economic and social institutions. Thomson is clearly at home in these areas. In the textbook, he has a chapter on "The Forces of Change," which begins with a section on the growth of population: "The speed of this growth was a completely new phenomenon. . . . No social and political order could have remained unaffected by so immense an increase of humanity; and the events of the nineteenth century remain unintelligible unless this greatest revolution of all is kept

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constantly in mind" (pp. 92 f.). Thomson notes that this increase "changed the course of world history by making it possible for the smallest of the continents to colonize a good part of the rest of the world."

Why this population explosion? Thomson stresses the fall in death rates: "Populations grew not so much because more people were born as because more survived, and more stayed alive longer" (p. 93). And why lower death rates? For four reasons that are very close to those of Palmer and Colton: "improvements in public order and security" effected by stronger monarchies, which ended civil and religious wars, repressed violence, and "did much to relieve famine, plague, and destitution"; advances in medical science in the eighteenth century, which freed Western Europe from "the worst endemic diseases and plagues that had taken a constant and heavy toll of all populations down to the seventeenth century"; improvements in transportation, which "made it possible to end localized famine and shortages"; and increases in food output. Unlike Palmer and Colton, however, Thomson dates the agricultural revolution from 1800 onward and treats it more as a response to population pressure than as a cause of growth.

Yet Thomson does less with the consequences of population growth than with its causes. Aside from this discussion of the agricultural revolution and an allusion to the implications of an expanding population for Russian territorial expansion, he contents himself with an omnibus model linking population and general change (p. 94):

> If nineteenth-century Europe appears in history as unusually restless, explosive, and prone to revolution, this remarkable demographic fact is at least one explanation. Against this tide no social and political order could stand intact. No mere "restoration" of old institutions and traditions could suffice to meet the needs of the new masses of humanity which so abruptly made their appearance on the old soil of Europe. Only constant inventiveness, reorganization, and experiment in new forms of social life could sustain civilization. In the sphere of economic production and distribution, this inventiveness and reorganization took the form of what is traditionally called the "industrial revolution"; in the sphere of social life and organization, it took the form of urbanism and, eventually, suburbanism.

Thomson has more to say about population in a later chapter on "The Balance of Power in Europe, 1850-70." Here he provides further statistics on population growth, which he then transposes into ambiguous rates of increase: German population grew "at the

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rate of 8 per cent"; that of France, "by roughly 7.5 per cent" (p. 228). The reference is apparently to decennial rates, but this is not indicated in the text. Neither is the reader provided with a table, again perhaps for fear of rendering the text illegible to anti-quantitative undergraduates.

Thomson then alludes briefly to urbanization, noting simply that as a result of the move to the cities, "the whole texture and fabric of social life was changing . . ., creating novel or more acute problems of urban housing, sanitation, public order, and organization" (p. 228). He returns to the topic in a chapter on "The Pattern of Parliamentary Democracy" in the prewar period. Again the discussion begins with a selection of statistics, with particular emphasis on differential rates of growth of various countries; this time rates are specified as per year, but there is still no table, much less a chart. Then the discussion moves on to the implications of urbanization, its political consequences in particular: "Its significance for internal politics was that every European government now had to administer and serve the interests of larger and denser agglomerations of people than ever before in the history of mankind" (p. 326). Thomson enumerates some of the tasks this entailed: "how to ensure adequate provision for public health and sanitation, public order, and police; how to protect industrial workers against bad conditions of working and living" (p. 228).

The subject of population comes up again at a number of points in the narrative: in regard to the implications of the divergent rates of increase of Germany and France for the international balance of power (p. 521); to the demographic consequences of World War I ("The French calculated that between August, 1914, and February, 1917, one Frenchman was killed every minute" [p. 537]); to the end of the great wave of European overseas migration ("The age of wide open spaces and boundless opportunities, of limitless expansion and dynamic productivity, had come to an end" [p. 656]); to the renewal of fertility in post-World War II Europe (p. 803); and to the drop in mortality rates during these years and the effect of continued population growth on the demand for food and raw materials. This dispersal of the material has its disadvantages; one misses, for example, a systematic discussion of the concluding phases of Europe's demographic transition. On the other hand, dispersal in the hands of someone like Thomson is an integrative device. He weaves population into the larger story and, by so doing, conveys more perhaps than if he encapsulated

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the subject and set it apart as a kind of esoteric special topic.

One could continue with other texts, with varying results. In general, the more narrow the focus on modern European history, the more sensitive the author is to the significance of population change. At the same time, the closer the author's specialization is to economic and social history, the more easily he handles demographic phenomena and the more space he is prepared to give to them. In a text, for example, like that of Blum, Cameron, and Barnes (two of whom are economic historians), population is treated in at least four distinct sections, as well as *passim;* and the authors do not shy away from the use of tables.[18] Whether demographers would be entirely satisfied with the authors' explanation of population growth, which takes the form of a timeless enumeration of determinants of varying specificity, is another matter.[19]

It is clear, then, that on balance the treatment of population in college texts has improved considerably over the last generation; but to some extent this improvement is implicit in the point of departure. The textbooks of the interwar period were almost invariably so inadequate in this regard that even a modicum of discussion looks good. When one compares the textbook treatment of population with the latest work in historical demography, it is clear that the authors of these manuals are inclined to rely on other general works or some of the older monographs—even on other textbooks; that they have little familiarity with the concerns and findings of the latest research; that they are usually awkward and often inaccurate in their citation and manipulation of numbers; and that they find it hard to state an explicit model, right or wrong, positive or conjectural, of the process of population change.

There will always be some gap between the treatment given in the textbooks and the work done on the frontier of the discipline. This is true of any topic. In the case of demography, however, the discrepancy is aggravated by the technical character of the research, which becomes more esoteric with time, and by the disinclination of teachers and students allergic to numbers.[20]

Obviously any effort to diminish this lag calls for work by both demographers and historians. The former have to find the time to write general studies of population that are comprehensible and relevant to the general historian; the latter have to learn to understand and deal with numbers and must be ready to insist that their students do the same.

Such a prescription is easy enough; its implementation is an-

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other matter. The operative word here is *relevant*. The readiness of historians to invest their own time in demographic studies and to offer instruction in this area to students will depend on the significance of these studies for an understanding of the general process of historical change. In this regard, the statements of a number of textbooks make it clear that some historians at least are already converted to the cause. These are, however, the exceptions, and even they are hard put to convey to the reader the significance of population change. The difficulty may be due in part to their own shortcomings, but I submit that it owes more to the limitations of demographic history, which has devoted almost all its efforts to the study of the determinants of population change and little to an examination of its consequences. For the general historian, it is the consequences that really count. He would like to say something about the causes of the increase of the eighteenth and nineteenth centuries—if only because historians like to say *why* as well as *what*. He will become convinced that all this is worth saying, however, only when it illuminates the larger story.

In this regard, as we have seen, there are a number of obvious implications of population change that historians have almost converted into clichés: the effect of differential rates of growth on the balance of power (France *vs.* Germany, Protestants *vs.* Catholics in Holland, Europe *vs.* the nonwhite world); the pressure of numbers on the food supply, and hence agriculture; the relation of Europe's population explosion to the great wave of overseas migration. These leave vast areas of social and economic demography unexplored. To cite a few: the effect of growth or stagnation of population on demand for goods, on the supply of labor, on the rate of economic growth; or the consequences of greater longevity for age distribution and the allocation of resources. For that matter, what are the general implications of the age distribution for the character of a society? What does it mean to be a young people or an old people? What does it mean to have an unbalanced sex ratio—as a result of war, for example? What are the psychological effects of very late marriage, or extensive celibacy, as in Ireland? (The novelists write about this, but historians have little to say.) What about differential mortality between rich and poor, urban and rural, between one occupation and another? One of the important works of postwar social history is Louis Chevalier's *Classes laborieuses et classes dangereuses*, which uses the differences in death and disease rates from one *arrondissement* of Paris to another to illuminate the

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social structure of the city, working these statistical data into a psychological analysis of the isolation of the lower classes.[21] For all Chevalier's originality, he is not the first to do this kind of thing; one can go as far back as Kay (later Kay-Shuttleworth), writing in 1832 in Manchester and offering the reader what amounts to a statistical map of urban pathology[22]; the approach recurs repeatedly in later British reports.

One could add considerably to this series of questions, but to no purpose. The point is that if textbooks leave much to be desired in this area, it is at least in part because the specialists have done so little with these aspects of the subject. In a conference like the American Academy one on Historical Population Studies, we have skirted these questions repeatedly—gone to the very edge and then stopped and confessed our ignorance. If we have done this, it seems to me, it is not for want of desire or curiosity. Answers to such questions are hard to come by, specifically because they do not lend themselves easily to the techniques of analysis that have been so successful in purely demographic research. To study the impact of population change on other aspects of historical change requires one to introduce extra-demographic variables, some of them recalcitrant to quantification.

Under the circumstances, what is needed is some kind of joint effort, combining the resources of demographic history with those of other branches of the historical profession and of allied disciplines. There is already a small body of work along these lines. Chevalier's study is one example; it is no coincidence that Chevalier had had close connections with the Institut d'Etudes Démographiques in Paris. The best French regional studies—Goubert's monograph on Beauvais and the Beauvaisis, Le Roy Ladurie's recent work on the peasants of Languedoc—build much of their analysis around a demographic core.[23] This historiographical tradition perhaps accounts for the place accorded population in some of the newer French general histories.[24] In Britain, the Cambridge Group for the History of Population and Social Structure, founded in 1964, have played the role of pioneers, and Peter Laslett's *World We Have Lost* (1965) and E. A. Wrigley's recent article on London's influence in the century before the Industrial Revolution are only the first fruits of what promises to be a bountiful harvest.[25] In central Europe, one thinks of the important and original volumes of Rudolf Braun on the industrialization of the Zürich highlands from the seventeenth century on.[26]

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This new approach, moreover, is infectious. Demography and demographic history have a strong comparative bent, and both emulation and collaboration work to promote the new techniques and directions of research. If the impact on general history has thus far been limited, it promises to increase rapidly. In 1965, Louis Henry, who presents himself emphatically as a pure demographer concerned with demographic variables, wrote that the progress in demographic research during the last two decades "could scarcely have been foreseen" fifteen or twenty years before.[27] Who is to say how far we will move toward integrating demographic history in general history in the next two?

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3. The most important German contribution to the more recent history of population was a series of monographs edited by F. J. Neumann, *Beiträge zur Geschichte der Bevölkerung in Deutschland seit dem Beginn des Jahrhunderts* (7 vols.; Tübingen, 1883-1903). The scope of the series was widely construed to include studies of handicraft industry and social welfare.

4. Griffith was preceded by E. C. K. Gonner, a student of agrarian history who published an article on the same subject, "The Population of England in the Eighteenth Century," in the *Journal of the Royal Statistical Society*, Vol. 76 (1913), pp. 261-303; and by John Brownlee, a statistician, who wrote among other things an essay on "The History of the Birth and Death Rates in England and Wales Taken as a Whole, from 1570 to the Present Time," *Public Health*, Vol. 29 (1916), pp. 211-22, 228-38. These

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quantitative studies, although laying the empirical foundation for Griffith and his successors, did not stimulate the kind of debate that arose the moment one moved from narration or description to explanation.

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6. See especially the work of J. A. Banks, *Prosperity and Parenthood: A Study of Family Planning Among the Victorian Middle Class* (London, 1954); also J. A. and Olive Banks, *Feminism and Family Planning in Victorian England* (Liverpool, 1964).

7. The most important innovation has been the method of reconstitution of families, which makes possible a far more effective exploitation of pre-census sources. The technique was pioneered by Louis Henry and his associates in the Institut National d'Etudes Démographiques in Paris, beginning in the 1940's, and has since spread to other countries, in particular to England. For the technique, see Louis Henry and Michel Fleury, *Nouveau manuel de dépouillement et d'exploitation de l'état civil ancien* (Paris, 1965); Louis Henry, *Manuel de démographie historique* (Paris, 1967); and Wrigley (ed.), *Introduction to English Historical Demography*.

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12. *The Industrial History of England,* pp. 269-71. Usher gives his source as the Census of 1821, but this is presumably a typographical error.

13. It is interesting to compare the textbook of 1920 with W. Bowden, M. Karpovich, and A. P. Usher, *An Economic History of Europe Since 1750* (New York, 1937). The latter gives considerable attention to population, especially in its economic aspects: distribution in space, relation of density to economic activity, comparative rates of growth—the whole supported by tables and semilog graphs. It does little with the components of these changes, saying nothing about birth rates or death rates, for example, and makes no real effort to account for them, leaving the reader to infer that increased resources permitted larger numbers. By increased resources, moreover, is meant coal more than food; economic activity (by implication, the demand for labor) more than means of nourishment. The result is a strangely simplistic model of growth. Thus the low rate of increase in France was "mainly attributable to losses in extensive areas which were not more than offset by the gains in the industrial and commercial regions" (p. 18). No explanation is given for the diminished rate of increase from the late-nineteenth century on, but the sharp drop in the twenties is imputed to "direct losses of life in the war, to the unfavorable age distribution of the postwar period, and to unfavorable economic circumstances."

14. The recent article of Razzell on the effects of inoculation against smallpox may have re-opened the issue. I say, "may have," because there are those who feel that Razzell's evidence tends to exaggerate the extent of the practice, in that it consists in part of self-advertising by practitioners of the new art. Still, the persistence of controversy says something about the limitations of our knowledge.

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15. Robert R. Kuczynski, *The Balance of Births and Deaths* (2 vols.; New York, 1928, 1931).

16. The first edition appeared in 1951. The second and third editions (1956 and 1965) are co-authored by Palmer and Joel Colton.

17. On the latter question, see especially the new book by Eric Kerridge, *The Agricultural Revolution* (London, 1967).

18. Jerome Blum, Rondo Cameron, and Thomas Barnes, *A History of the European World* (Boston, 1966).

19. "The growth in population was due principally to industrialization and its concomitants, such as agrarian reform and the increase in agricultural productivity. The improvements in transportation and communication and in medical science and sanitation contributed to the growth of population even in areas that did not experience industrialization directly, sometimes with dire consequences" (pp. 562 f.).

20. See, for example, on the techniques coming into use in demography proper, *Emerging Techniques in Population Research* (Milbank Memorial Fund, 1963). As for historical demography, the proceedings of the present conference are themselves evidence of the rapidity with which methods and terminology are changing in an esoteric direction. This tendency, needless to say, is not to be criticized in itself; all developing sciences take this path. Nevertheless, esoteric technique does constitute a barrier to communication with other disciplines, especially a discipline like history, which retains a strong humanistic, nonquantitative component.

21. Louis Chevalier, *Classes laborieuses et classes dangereuses à Paris pendant la première moitié du XIX^e siècle* (Paris, 1958).

22. James P. Kay, *The Moral and Physical Condition of the Working Classes Employed in the Cotton Manufacture of Manchester* (London, 1832).

23. Emmanuel Le Roy Ladurie, *Les paysans de Languedoc* (2 vols.; Paris, 1966).

24. See, for example, Jacques Godechot, "Le siècle des lumières," in R. Grousset and Emile G. Léonard (eds.), *Encyclopédie de la Pléiade: Histoire universelle*, Vol. 3: *De la réforme à nos jours* (Paris, 1958), pp. 288 *et seq.*

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27. In the Preface to Wrigley (ed.), *Introduction to English Historical Demography*, p. vii.

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LOUIS HENRY

Historical Demography

When I agreed to write this article for *Dædalus*, I planned to devote it to current studies at the Institut National d'Etudes Démographiques and to provide a summary of other French work in the field. Being a demographer and not a historian, I thought I would also be able to place historical demography within the field of demography as a whole: to discuss those problems which are unique to it and those it shares with the entire discipline. The article thus consists of two parts, which although not closely interrelated are not altogether independent of each other. My purpose is to analyze the place of historical demography within the wider discipline of demography itself and to consider French studies in the field, with emphasis on those of L'Institut National d'Etudes Démographiques (I.N.E.D.).

The Field of Historical Demography

When I first became interested in historical demography about fifteen years ago, the object of study was the analysis of populations which existed before the advent of demographic statistics. In France, for example, this meant that historical demography was concerned with population studies before 1801, the date usually held to inaugurate the statistical era. The Census of 1801 was considered more reliable than those which had preceded it during the Revolution; 1801 was also the first year of an uninterrupted series of figures recording the movement of population.

This definition of the boundaries of historical demography remains true, but only approximately. The prestatistical period forms far from a uniform whole, and it is necessary to distinguish a number of separate elements within it. The first statistics resemble,

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moreover, a harvest so summarily gathered that more grain remains to be garnered than has as yet been stored. These statistics were often collected with other purposes in mind. Vital records furnish a typical example.

In France, registration of baptisms, marriages, and burials was initiated in the diocese of Nantes as early as the beginning of the fifteenth century. The practice then spread to neighboring dioceses, and several series of registers exist from the second half of the fifteenth century. By the end of the fifteenth century and the beginning of the sixteenth, a concern with keeping records is found in several widely separated dioceses—Normandy, Paris, Franche-Comté, Provence. In 1539, a royal edict obliged the entire kingdom to register all baptisms and the deaths of those persons holding ecclesiastical benefices. The registration of marriages became obligatory in 1579. (This edict served mainly to reinforce that of the Council of Trent, passed in 1563, whereby curés were enjoined to register all baptisms and marriages.) Finally, the registration of deaths was required by Pope Paul V in 1614.

In 1667, a royal ordinance standardized the registration of baptisms, marriages, and burials; its measures were revised, modified, and reinforced in 1736. An edict of 1787 gave non-Catholics the right (which had been suppressed in 1685) to be registered separately. The corresponding registers were, in principle, to be maintained by the judges, but were, in fact, recorded by denominational leaders. In September, 1792, registration was transferred from the curés to the mayors. The law was not substantially altered in other respects, and continuity was maintained. The old parish records were transferred to the mayor's office, and today many town halls, especially in the villages, do not distinguish between the pre-Revolutionary parochial registers deposited there and the true register of vital statistics that succeeded them.

In regard to vital statistics, the prestatistical era in France is divided into three parts: an early period, for which few, if any, statistics exist; an intermediate period for which registration is incomplete and imperfect; and a recent period at the beginning of which registration approaches modern standards and at the end has attained them—most often in the years following the law of 1736.

Other documents besides those recording vital statistics also exist: tax rolls, listings of inhabitants, and so forth. Complete listings of inhabitants are rare, but in the eighteenth century lists for the

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taille are abundant and frequently form a continuous series. Their existence, however, does not modify the division of periods established above in respect to vital statistics; furthermore, this division can be applied to all European countries (correcting for differences in dates necessitated by an earlier or later start in registration and the time when this registration became more exact).

For demography, the earliest period is a kind of prehistory, beginning with the origins of humanity and extending to the dawn of modern times. Research in it is not impossible, but its methods are very different from those of classical demography and often belong to archaeology. The risks, as in archaeology, arise from superficial generalization from unrepresentative remains. When, however, a study of a population is joined with a knowledge of its way of life, plausible figures can be established and its distribution within a territory indicated.

The intermediate period appears to offer more possibilities. It is, however, perhaps less studied than the early period, if one excepts some monographs devoted to documents of exceptional quality. No specific methodology exists for the intermediate period, and nothing presently indicates that one will be found.

The situation is completely different for the recent period. An exact methodology exists and when carefully adhered to, it endows those interested in historical demography with a precise tool from which promising results have already been obtained. The lines between historical and classical demography have, therefore, been considerably modified; historical demography is now more and more concerned with the beginning of the statistical era, still so little known. In France, for example, historical demography tends to stretch back as far into the nineteenth century as the rules governing access to documents permit. Because vital statistics fall into the public domain only after a hundred years, 1866 constitutes the present frontier of historical demography.

Historical Demography and Classical Demography

The history of demography, since the start of the science in 1662, has been marked by two preoccupations. The first demographers concentrated on the study of mortality to such an extent that the first century of demography—from John Graunt to Per Wargentin—consists of the progressive discovery of correct procedures for the measurement of current mortality. In the nineteenth

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century, official statistical services were created to record the size and movement of population. The information and knowledge thereby acquired had both an administrative and scientific utility.

These two facts are not independent of each other. A study of mortality is hardly possible without a census which records the number of people and their classification by age. When, at a very late date, demographers turned their attention to the study of other demographic phenomena—birth and marriage—they modeled this study on the earlier analyses of mortality, without questioning too deeply whether it was necessary to proceed in this way. As a result, in demography everything was studied by a judicious combination of the data extracted from two sources: the census and vital statistics. When vital statistics failed to provide certain information, they were supplemented by the census, so that the latter became the keystone in the arch of the demographic edifice.

Because a census could be taken only by a statistical service, this service had a monopoly on statistical observation—that is to say, a monopoly on demography itself. Until the last war, all that was not classical demography was marginal and without real importance. The lines of thought that classical demography followed often led to impasses because its methods were too rigorously focused on the present—on events of the moment. At the time, however, no one was concerned that it should be otherwise.

After 1945, the situation was modified by several changes whose repercussions will be felt for a long time. In the United States and Britain, the analysis of demographic phenomena by periods (transversal analysis) was thought to be ill-suited to phenomena other than mortality. Demographers in Britain and the United States, thus, substituted an analysis based on the experience of each generation or cohort throughout its lifetime (longitudinal analysis) for transversal analysis. Today longitudinal analysis has largely conquered demographic studies; even mortality, which has so far escaped, begins to be menaced.

Reflection on the procedures of statistical observation has made the need of the double source of data—the census and vital statistics—somewhat less imperative. It was seen that, for the past, almost everything required could be found from vital statistics alone, provided they were accurate. It became apparent, even for studies of the present, that it was not possible to answer all the needs of demography by combining the data of the census and the vital statistics.

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A new means of statistical observation—sample surveys—came into wide use. Although often conceived simply as a complementary procedure, the sample survey offers new possibilities and puts demographic observation within the reach of more organizations (for example, universities and research institutes). The *de facto* monopoly of the statistical services can no longer be justified.

Requirements for information and opinions in the field of economics and demography have increased so sharply within government that the statistical services have become more and more occupied by current events. Providing data has clearly become, at the expense of scientific analysis, the predominant focus of statistical services. The disequilibrium between collection and analysis has led to an almost total disinterest in the past, even when it belongs to the statistical era.

Since the war, permanent or temporary organizations for research have been created or launched. These organizations do not pose demographic problems in the same terms as the statistical services. Scientific concerns predominate; the preoccupation with events of the moment is diminished.

Such changes have great importance for historical demography. Longitudinal analysis is naturally inclined toward a historical perspective. The importance it has acquired tends to make all demography a history of successive generations. Historical demography, which quite naturally uses longitudinal analysis, therefore became an integral part of demography as a whole, just as the history of the eighteenth century is an integral part of history as a whole.

To fail to observe demographic phenomena in the manner of the statistical services is no longer considered erroneous; moreover, historical demography is now completely independent of the statistical service and is free to pursue its observations in terms of its needs, taking into account all relevant documents. No other branch of demography now benefits from an equal autonomy.

The diminished participation of the statistical service in the development of demography as a science minimizes the importance of purely contemporary events. As a result of this evolution, the frontier between historical demography and current demography has been reduced, if not eradicated.

Demographic phenomena are inscribed in time. Such phenomena cannot be explained nor understood unless they have been traced through the concatenations of many decades or centuries,

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as far back as available observations and documents permit us to go. To study demography only from current events is equivalent to the study of astronomy without benefit of earlier observation or to the construction of a theory of evolution with attention to none but presently living species. Can one imagine a meteorology which did away with the information of the last century under the pretext that it was no longer current?

We do not know whether laws analogous to those which regulate natural phenomena exist for demography, but what we do know shows that any future cannot proceed from a given present. If there are no permanent and rigid statistics in demography, there are certainly relations among phenomena. By studying these, we can hope to improve our knowledge and our ability to forecast. Only the observation of as long a chronological series as is possible will furnish all the relations observable up to the present. In demography, therefore, the important factor is not to possess the most recent information about the population of a certain country or city, but to be able to dispose of homogeneous retrospective statistics extending as far into the past as possible. If this is accepted, it is hardly a paradox to say that historical demography is all demography—or, to put it another way, that from the scientific point of view demography has as its object the study of all observable populations, past and present. Given the actual state of affairs, however, I prefer the first formula, for it focuses more clearly on the primary importance of time.

The paradox to which I subscribe is not inspired by a particular love of the past. I am not a historian, and I came to historical demography because I needed information on natural fertility (fertility unlimited by birth control). Because historical demography furnished this information, it was possible to advance the study of biological factors in fertility and to construct a model that could serve as a guide to biologists in certain studies of the physiology of reproduction. As a demographer, I also know that modern statistics do not furnish so much diverse information as can be collected from the study of rural families of the eighteenth century. This lack could easily be corrected by a few sample surveys—that is, if one gives, finally, more weight to studies in depth than to peripheral current happenings.

Historical Demography in France

Historical demography in France was launched less than fifteen

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years ago. As always, it is difficult to say after the fact exactly what role each person and each organization played. Interest in social history existed before this date, and certain scholars recognized that demographic phenomena should be taken into historical account. This was true of C. E. Labrousee, L. Chevalier, J. Meuvret, M. Reinhard, and of the Ecole Pratique des Hautes Etudes. Echoes of the idea may be found in *Population,* the journal of the Institut National d'Etudes Démographiques, although the Institut was not generally concerned with historical research. The real impulse came later, from Pierre Goubert in his study of social history, *Beauvais et le Beauvaisis de 1600 à 1730,* in which demography played an important part. An article written in the *Annales* inspired me to write an article for *Population* in 1953. This, in turn, attracted the attention of archivists, that of M. Fleury, in particular. Several years later, he and I edited *Manuel de dépouillement et d'exploitation de l'état civil ancien.* M. Fleury's role was not limited to collaboration in this work. He pointed out to me the value of the Genovese geneologies as a source of demographic research on a governing class from the middle of the sixteenth century until the present.

At about the same time I made the acquaintance of E. Gautier. He had already reconstituted the families which had issued from marriages celebrated between 1674 and 1742 in Crulai, a Norman village where his family originated. Several years later, we published a monograph that furnished a methodology not only for similar monographs, but for more extended studies.

This monograph was followed by several others, due for the most part to historians P. Girard and M. Terrisse, students of M. Reinhard. P. Girard studied Sotteville-lés-Rouen, and M. Terrisse worked on Ingouville. J. Ganiage, after having made a study of Europeans in Tunis in the nineteenth century, presented a monograph on a group of three villages in the Ile-de-France. This study indicates that fertility declined in these villages at the time of the Revolution, if not somewhat earlier. P. Chaunu, a professor at the Faculty of Letters at Caen, started his students and collaborators to work on monographs and then on more extended studies; the first finished was by P. Gouhier and discusses a Norman port, Port-en-Bessin.

In the South of France, J. Godechot, Dean of the Faculty of Letters at Toulouse, also set his students to work on monographs. For its part, I.N.E.D. published a monograph on two villages of the

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Lot by one of its former collaborators, historian P. Valmary. P. Goubert directed an extended study in the sixth section of the Ecole Pratique des Hautes Etudes.

I will cease this enumeration, which, although incomplete, may be tedious. It does, however, demonstrate that historical demography in France began with abundant monographs on individual villages. Although these do not constitute a strict sample of the rural population of the past, they are similar enough to provide the broad outlines of what might have been the demographic characteristics of French peasants in the eighteenth century. These are late marriage (an average of age twenty-five for women and twenty-seven to twenty-eight for men); a low proportion of older unmarried people; high marital fertility (four or five children per marriage, but six or seven children for the marriages of completed fertility); low total sterility; average interval between births on the order of 2 to 2.5 years while the woman is fairly young; low illegitimate birth rate; and frequency of premarital conception variable among regions. Infant mortality was high (20 per cent to 25 per cent), due, in large part, to diseases of the newborn (8 to 10 per cent). The mortality rate for children of one to four years, and even for children of five to nine years, was also high. Only about 60 per cent of those born survived to age fifteen. There was a life expectancy of about thirty years. Results on the subject of migration are few, although certain monographs suggest a continuous and not negligible stream of departures, either for other regions or, more probably, for the cities.

Because of the difficulties of observation, no studies presently exist for classes outside the rural population, except the "Dukes and Peers." The study of this part of the nobility indicates the early appearance of some form of birth control. Already obvious in the eighteenth century, birth control had probably existed as early as the end of the seventeenth century. In rural areas, a reduction in fertility occurs much later; it was evidently practiced by the time of the Revolution. Authors of the period say that birth control appeared earlier, but this remains to be proven statistically.

The preparation of monographs continues, and these will be of interest for some time even if they do nothing more than confirm the expected results. Meanwhile, research on a much larger scale has appeared. Cities, regions, France as a whole are the subjects of new studies.

Chronologically, the study of France as a whole was undertaken

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first and covers the period 1670 to 1829. Vital statistics are taken from the registers of a random sample of rural communes and villages. From these data, the births, marriages, and deaths of the country are estimated. As the age of death is almost always known at least approximately, it is possible to reconstitute the population by sex and age. The demographer starts with the classification of the population taken from a census year in the nineteenth century, and goes back in time so long as the registration of deaths exists. This procedure had already been used by J. Bougeois-Pichat for the beginning of the nineteenth century; it will be necessary to continue it after the results already obtained are verified. Once the population by sex and by age is known, one can determine the mortality levels for every five or ten years.

Since the population of France as a whole was basically closed, the quality of the registration, that of deaths in particular, can be checked by comparing the total number of deaths of one generation to its initial size. If registration has been exact, the two numbers should be equal. If there is under-registration of deaths—which is often the case before 1740—the initial size is above the total registration of deaths. If the difference is not great, it can be attributed to the under-registration (also frequent before 1740) of infant deaths.

The work in question was undertaken in 1959 by I.N.E.D. and covers about thirty towns and 375 rural communities. The data have been collected, and collation is now in process for the period 1740 to 1829 only. For the purposes of this study, France has been divided into ten regions; as yet, only one region is finished—the extreme west of the country, Brittany-Anjou. Progress is slow because it is impossible to apply gross measures in a study of this kind; with this reservation, the collation is proceeding normally.

In forty of the 375 villages, a nominative extraction (that is, identification of people by name) has been made in order to permit a study of fertility based on the reconstitution of families. The forty villages represent all parts of France and constitute a random sample of the forty thousand rural parishes. The reconstitution of families is well advanced, and it will soon be possible to begin the collation for the northern part of France.

To many, the reconstitution of families seems a particularly arduous task. This impression arises, in particular, from a lack of funds and, therefore, of personnel. In reality, however, the reconstitution of families does not demand much more time than the

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nominative extraction. The combination of the two operations represents about ten thousand hours of work for the forty villages, approximately a year's work for five full-time persons. This is little effort if the purpose is to assemble the basic givens of a country for one hundred and sixty years. Especially is this so if one compares this effort with that demanded by the collection of facts now being made for the modern period.

The first regional study was launched by J. Dupâquier, who was then in charge of research at the Centre National de la Recherche Scientifique and is now Assistant at the Sorbonne. This study deals with the French Vexin, a region near Paris which includes about ninety parishes. A nominative extraction is being made only for those individuals whose last name begins with the letter B and for women having or having had a husband whose last name begins with B. This study should allow us to recognize those families who move from village to village and to see if their demographic characteristics, and their fertility in particular, differ from those of the families who stay in one place. Until now the monographs on villages have concentrated on families with no mobility.

An analogous study, but one with an integral extraction of facts, is going to be undertaken for a small and fairly closed region of Normandy under the auspices of P. Chaunu. Chaunu also plans a complete analysis, with reconstitution of families, of Rouen. This will be the first large-scale attempt to study in depth the urban demography of the past. A smaller study, on Caen, has been done by J. C. Perrot.

These two studies are part of a large program of research that will be undertaken in 1968 with the support of the Centre National de la Recherche Scientifique. The proposed program is as follows:

1. A systematic study of the people of France from the eighteenth century to the present, with the establishment of a national file (administrative, demographic, and economic history) of parishes and communes.

2. An effort to use the registers of the sixteenth century for regions which possess the greatest number of such registers.

3. An extension, as far as 1870, of the nominative extraction and the reconstitution of families in the forty villages of the I.N.E.D. sample.

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4. A study of the causes of death before the time of Pasteur, from various documents heretofore not consulted.

The studies previously mentioned have been based on vital statistics; to conclude this survey of demography, I should like to mention studies based primarily on lists. J. Dupâquier has recently shown what can be extracted from tax rolls in regions where an adequate series of them exists. Within the framework of a village monograph, Y. Blayo has undertaken the systematic comparison of nominative lists of the census between 1836 and 1861 in order to discover who left and who arrived. This work is not too different from what can be accomplished with two seventeenth-century nominative lists of an English village.

Certain studies have been undertaken that encompass demography, economics, and social life. That of E. Le Roy Ladurie on the Languedoc peasants from the fifteenth to the eighteenth century merits special mention because it highlights man's relationship to the earth in an agricultural economy when all cultivatable space is occupied and productivity does not change.

The vitality of historical demography in France is also signalized by the creation in 1962 of the Société de démographie historique, which has an international field of interest. This society publishes yearly a collection of studies and chronicles; three have already appeared.

At the risk of making this article a plea for historical demography, I stress in conclusion only these points. Contrary to what a superficial view might suggest, historical demography is not simply a marginal part of demography. It is demography itself, just as demography, being a study of human populations in time, is history itself.

Historical demography now has the enormous advantage of not being dependent on any organization for its observations. In spite of the evident difficulty caused by this independence, the advantages are fundamental to many of the recent successes of historical demography.

In certain countries, historical demography is today one of the most vital branches of demography; paradoxically, it is also one of the most modern. Historical demography has, in fact, been most marked by the postwar developments in the methodology of dealing with demographic phenomena. Historical demography's in-

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dependence in regard to the statistical services has enabled it to adapt more successfully to modern conceptions.

Finally, at a time when one often speaks of the collaboration among scientific disciplines, frequently without making much effort to bring it about, it is a pleasure to point out that the collaboration between historians and demographers is fairly well guaranteed, in spite of considerable differences in their formation and preoccupation. It is a good augury.

This article was translated from the French by Patricia Cumming.

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J. A. BANKS

Historical Sociology and the Study of Population

I

MODERN SOCIOLOGY is scientific in intention, if not always in execution. As John Madge phrased it recently in his account of the subject over the past fifty years: "The discipline of sociology is at last growing up and is within reach of attaining the status of a science."[1] The point of his argument is that since the turn of the century sociologists have slowly and painfully developed techniques and concepts for tackling the special problems of their subject, and that they have at last begun to emancipate themselves from the stultifying effects of the debate about whether the social sciences can or cannot be logically assimilated into the natural sciences. Indeed, the writers treated in Madge's study ignored the controversy altogether. They set out instead to tackle a limited set of problems with whatever tools they could fashion for the purpose—content analysis, participant observation, interviewing, experiment—and from the mistakes they made in the process, they obtained a clearer grasp of what the sociological adventure is all about. Of course, at no time did they abandon what has been said to be the chief objective of a science—namely, "to explain the phenomena in the world of experience, to answer the question 'why?' rather than only the question 'what?'"[2] In this sense, they may be said always to have had in mind the eventual establishment of scientific laws, true propositions about uniformities, universals independent of space and time.

In the present context, it is instructive to notice that not one of the twelve themes reviewed by Madge was at all historical in the way in which the writings of Comte, Spencer, Veblen, Sumner, and Hobhouse were historical—that is, in applying some kind of evolutionary or developmental conceptual framework to the history of

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mankind and using evidence in the form of a variety of events from the past to illustrate it. Historical sociology of this bent, although still extant as the case of Pitirim Sorokin exemplifies, has in very recent times been superseded by a newer conception of historical sociology, more consonant in its methods and objectives with that sociology which applies scientific procedures to the study of current data. Such historical sociology uses what has been called the hypothetical deductive method[3] for the formulation and testing of hypotheses, and only differs from other scientific sociology in that its data are documents and artifacts, taken as indications of past events, rather than as observations of and answers to questions put to people at the present time. In principle, there is no logical distinction between these two procedures, since the testing of universal explanatory generalizations can be carried out through the systematic examination of documents, artifacts, observations, and questionnaire responses indiscriminately. The decision whether to use one or more kinds of data is merely a matter of convenience, although in some circumstances there may be differences in the levels of objectivity and precision which they offer.

Nowadays a clear distinction may be drawn between historical sociology and the sociology of the older sort as well as the history pursued by professional historians. What separates them, indeed, is a differentiation of aim. Although both science and history make use of the particular as well as of the general, the former regards particular instances as merely the means by which one can arrive at general laws, whereas the latter does not regard the establishment of such laws as part of its province. As Ernest Nagel has explained:

> It is unlikely that anyone would find something radically wrong with a treatise on theoretical thermodynamics which did not contain a single proper name or a single reference to any particular date. But it is even more unlikely that anyone using the word "history" in its customary meaning would classify a book as a *history* if it mentioned no particular individuals, times, and places but stated only generalizations about human behavior. The distinction between history and theoretical science is thus fairly analogous to the difference between geology and physics, or between medical diagnosis and physiology. A geologist seeks to ascertain, for example, the sequential order of geological formations, and he is able to do so in part by applying various physical laws to his materials of study; but it is not the geologist's task *qua* geologist to establish the laws of mechanics or of radioactive disintegration which he employs in his investigations.[4]

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Historians may employ generalizations of a sociological or psychological order to account for particular occurrences, and in doing so they may make it plain that such generalizations are of limited validity, but they do not set out deliberately to test hypotheses in this fashion.

An example from the field of population studies will serve to illustrate the point. In his discussion of the economic and population histories of eighteenth-century Britain, H. J. Habakkuk has pointed out the unsatisfactory nature of the baptism and burial register records which the earlier historians, Brownlee and Griffith, had used to estimate the total population and the birth and death rates through the century. "One result of the paucity of statistics," Habakkuk asserts, "is that we cannot test explanatory hypotheses and weed out from among the many that are plausible those that fit the facts." At first sight this looks like a hypothetical deductive statement, but then Habakkuk continues: "Worse still, it is often not clear what precisely it is that has to be explained. How rapidly, for example, did population grow during these expansion periods?"[5] In the present context, the question is of considerable interest. What it asserts is the perfectly valid principle of historical logic that no useful purpose is served by putting forward plausible hypotheses to explain the "facts" when we do not know what the facts are. This is the very obverse of the procedure of the hypothetical deductive method in science—namely, that the search for the facts is a consequence of the formulation of hypotheses. In such a case, of course, the point of emphasis would not be the unsatisfactory state of eighteenth-century British records, but the need to look for records of some other time or place for more reliable data with which to verify or falsify the hypotheses. Habakkuk emphasizes, by contrast, the pioneer work of Chambers and Eversley in analyzing parish registers, and of Hollingsworth in examining genealogical trees. "Another decade or so of work on these lines," he thinks, will make it possible for us "to generalize with confidence about the nature of population change in this period."[6] It is primarily the facts and only secondarily their explanation which interest the historian. The scientifically-minded historical sociologist, on the other hand, sees significance in the reverse order of priorities.

There is, it should be understood, no ultimate evaluation implied in this contrast. Interest in general laws is no more or no less worthy a human trait than curiosity about and concern for specific facts. Indeed, the approaches of the historical sociologist and the

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historian may fruitfully supplement each other, often to the benefit of both. The former may indicate to the latter—largely by referring to tests which have been made with data from times and places other than those on which the historian happens to be concentrating—ways in which valid restrictions on the number of apparently plausible generalizations may be introduced; while the latter may be able to place the data in an economic, political, and social context drawn from systematic knowledge, a context which the former cannot possibly formulate in his concern for only those facts which seem directly relevant to the hypotheses he has in mind. The importance of this contextual knowledge can hardly be overestimated. As Karl Popper has emphasized, the scientific method of testing hypotheses depends not only on the results of controlled experiments or on the accuracy of observations, but also on whatever assumptions are involved in the process of deducing the one from the other. What he calls the "prognosis," the conclusion of the deduction which it is necessary to check against the facts, is derived from a combination of the hypothesis to be tested and "some other statements which for this purpose are not considered as problematic —for example, some initial conditions."[7] In the treatment of social fact, the quality of the assumption about contextual background can make or mar the relevance of data as a test of generalizations, because such assumptions usually provide Popper's "initial conditions," leading to the deduction of a prognosis. Indeed, the demonstration that evolutionist sociologists and social anthropologists had distorted evidence by treating it out of context gave the functionalist school much of its initial advantage as an alternative approach to the study of societies.[8] Contemporary historical sociologists, anxious to avoid the errors of their predecessors, try to work on a much narrower canvas, such as the relationship between economic institutions and the quality of family life in the Lancashire cotton industry from 1770 to 1840.[9] They still rely to a considerable extent, however, on secondary sources, the best work they can find by professional historians on the same time and place.

That the benefit may also work in the reverse direction may perhaps be most clearly illustrated by reference to what William Robinson called "the ecological fallacy," the assumption that associations computed from the means or other measures and derived from data about sets of persons are valid estimates of the associations that would have been obtained had the data been about individuals.[10] In an examination of Emile Durkheim's classic treatise on

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suicide, first published in 1897 and regarded by Madge as the pioneer work in the development of scientific sociology,[11] it has been pointed out that he regularly slipped into this fallacy.[12] For example, from a table on rates of suicide from 1878 to 1887 in French *départements,* classified according to the average number of persons of independent means living in those *départements,* Durkheim drew the correct conclusion that "the more people there are who have independent means, the more numerous are suicides."[13] But in employing this material, he intended to provide evidence for what he called "anomic suicide"—that is, suicide resulting from the fact that an *individual's* life has come to lack regulation.[14] The full rationale behind this argument—that wealth deceives its possessors into believing that their success is inevitable and unlimited, and hence predisposes them to suicide whenever crises threaten—need not concern us here. What is important is Durkheim's bland assumption that it was the wealthier members of *départements* whose suicides inflated the figures in his table or, alternatively, that the proportion of persons of independent means in a *département* at that time was a clear indication of the proportion of people in the *département* experiencing *anomie* and threatened with crisis. Neither of these assumptions was necessarily false, but the evidence presented cannot be applied to the hypothesis tested—that *anomie* is a cause of suicide—without the intervention of initial conditions, knowledge of which is highly problematical. Consequently, if a historian were seeking hypotheses to explain the kinds of data presented in Durkheim's table, he would be well advised not to formulate those which relied on assertions about the circumstances of individuals unless he had access to other information not apparently available to Durkheim.

The relevance of this example to the study of population should be evident because giving birth to a baby is as much an example of individual behavior as suicide is. At the present time, the Office of Population Research at Princeton is engaged in the formidable project of recording fertility in Europe using about seven hundred subnational units—provinces, *départements,* counties—rather than relying on the traditional practice of analysis by countries. The enterprise has been undertaken, in part, as a test of the hypothesis that there is an association between the process of modernization and the decline of fertility. There is also a subsidiary purpose to be achieved in the drawing of graphs "to display the spread of low fertility from certain focal points."[15] A historical sociologist would

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want the hypotheses underlying such work to be spelled out in much greater detail. The evidence cannot be interpreted as the behavior of individuals in any other sense than as residents in an area which, presumably, exerts some *collective pressure* that predisposes them to control, or not to control, their desire to procreate. Nevertheless, what kinds of hypotheses about the degree of urbanization, the occupational distribution of employment, the religious involvement of the inhabitants, or their political commitment can be formulated that will make good sociological sense when related to so nebulous a concept as residence?

This question should not be understood as implying that because fertility is a matter of individual decision or, rather, of the joint decision of two persons, data about couples are the only valid data which can be used to test hypotheses in this field. Sociologists interviewing a large number of families find themselves perforce obliged to group their data; often they have no alternative but to deal with summaries of aggregates provided by other people. Still, awareness of the ecological fallacy is a timely reminder always to bear in mind the implications of the methods of aggregation that have been employed. The Princeton study will have figures about the fertility of residents in regions. It will also have other kinds of data about these regions. For example, some regions will no doubt be more urban than others, as measured by the proportion of town-dwellers. Assertions about urban-rural differences in fertility, however, can only be tested by reference to urban-rural aggregates per se. Strictly speaking, any differences in fertility between more urban and less urban regions should be interpreted in terms of some *ecological* notion about the influence of urbanism on the region as a whole. This might further require a detailed study of some contemporary regions *ab initio*, rather than more elaborate analysis of existing demographic data; it is reasonably safe to conclude that what is true of urbanism will also be true of the division of labor, religious affiliation, political observances within regions, and other gross measures of social fact.

What contribution has the historical sociologist to make to the solution of such problems? In the ordinary course of events, he would not be concerned with this question, since it implies that he starts with fact rather than theory. He might, however, be required to face it were information about regions the only kinds of data available for the particular time and place he was studying. In such a circumstance, he would normally fall back

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on general sociological approaches to the study of, say, urbanism, although it is likely that in his final treatment of the subject he would not explicitly specify these approaches in detail. For example, he is almost certain to be aware of a strand current in urban sociology from at least the time of Durkheim which has regarded the higher physical densities of urban areas as having important consequences for social relationships. Durkheim, indeed, distinguished between physical density (*densité materielle*) and dynamic or moral density (*densité dynamique ou morale*), and argued that each was intimately connected with the other. In a series of deductions from the assumption that individuals have "a need" for as intimate a social contact with their fellows as they can find, he demonstrated that a denser population increases the opportunity of greater intimacy and also makes possible a diversification of interests and functions in social life. Such diversification, he concluded, occurs in the form of economic, political, and social specialization.[16] Empirical justification for Durkheim's point of view is, to be sure, difficult to obtain except in a broad sense, although more recent work on densities, markets, innovation, communication, and diversity and deviance among urban populations[17] suggests that, as an assumption about initial conditions, the degree of social contact, the nature of communications in a region, and possibly the normative patterns associated with these phenomena (*densité morale?*) might be taken as ecological factors responsible for variations in the attitudes about the size of family deemed satisfactory for a civilized existence, and hence for differences in fertility.

Save where this assumption might raise difficulties in other parts of his analysis, the historical sociologist could be content with the broad notion that regional data would be satisfactory for his purpose, at least until this assumption could be shown to be false for the period he was treating. He might, however, weigh this assumption against the conclusion, gleaned from a review "of the fluctuations, divergencies, and wide inter-area and inter-city differences in urban fertility," that urbanization per se is not an important determinant of fertility.[18] Thus, he might decide that it would be unwise to interpolate from city to region. To this day, sociologists do not really have much certain knowledge of what Louis Wirth called "urbanism as a way of life"[19]; indeed, some of the effects which thirty years ago seemed to be rather obvious features of changes from rural to urban are now thought to be rather more general aspects of change in *both* rural *and* urban.[20] At best, urbanism

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may be regarded as an intermediate rather than a determining variable in fertility decline. The sociologist must, therefore, consider the region in terms of assumptions other than those which have been used to account for differences between town and country.

Much the same kind of consideration applies to other possible characteristics of regions for which data may be available. Some evidence exists, for example, to support the view that fertility varies with what has been called "the future time perspective"[21] of individuals and that this perspective varies from occupation to occupation, possibly because of the nature of the relationship between an individual's work (thought of as a life chance) and the time factor. Certainly this hypothesis fits the case of nineteenth-century England rather well.[22] A classification of regions by the relative distribution of different occupations, therefore, might be regarded as equivalent to classification by variations in the future time perspectives of the residents. Unfortunately, this implies rather more knowledge about the nature of occupations than we possess. It also confronts the problem that a future time perspective is likely to be an ingredient of religious beliefs—as Beshers, following Weber, suggests. Here the sociologist is aware that at least the Protestant-Catholic dichotomy, or the Protestant-Catholic-Jew trichotomy, is unsatisfactory in many ways. Gerhard Lenski, for instance, has found no distinction among white Protestants in their attitudes to birth control, but marked distinctions among white Catholics when they were further subdivided into activists, regulars, irregulars, and marginals in their degree of involvement in church activities, as measured by their response to questions about regularity of church attendance and activity in church-related organizations.[23] The mere classification of a region as Protestant or Catholic, or as predominantly Protestant or Catholic, is a guide neither to the intensity of religious commitment there nor to the nature of the prevailing attitudes about time. Without this information, it is difficult for the sociologist to develop the kinds of unproblematical statements which can serve as initial conditions and a satisfactory bridge between hypotheses and data. The overworked *ceteris paribus* is no substitute for knowledge here.

II

All this may read as a counsel of despair rather than as a contribution to the study of population. Indeed, the immensity of the

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task of evaluating historical evidence in sociological terms has no doubt been one of the reasons why sociologists of this century have turned more and more to conducting surveys *ab initio* in order to test hypotheses. The task of working through other people's findings, with little knowledge of what they mean, has seemed far less profitable than it apparently appeared to previous generations of sociologists, possibly because more sophisticated techniques have promised better and more easily attained results from original work. To the sociologist, registration data on births and their subsequent conversion into fertility ratios leave much to be desired in this respect, primarily because there is little recorded information about the situations in which these births took place. Census and similar records do not enable one to distinguish between birth differentials due to variations in the natural fecundity of couples, their opportunity to copulate, and the effective use of birth control, or any mixture of these. Yet information of this kind is required if the study of fertility decline is to be related to biological and social variables. As a factor in the total birth rate, natural fecundity is probably too insignificant to influence differentials, since involuntary childlessness probably accounts for little more than 8.5 to 9.5 per cent.[24] On the other hand, there is some evidence that social mobility through marriage occurs for women who possess initial "advantages in growth, health, and education." Moreover, these women are "more efficient at child-bearing than women less well endowed"—that is, prematurity rates and obstetric rates are lower for such women.[25] If these biological (?) facts also influence fecundity rates, social selection may act to produce variations in the fertility rates of different social classes. In the theory of demographic transition, of course, the variations referred to run in the opposite direction from that which this hypothesis suggests, but they may nevertheless have operated to keep the differences among the classes *lower* than they might otherwise have been. Moreover, recent examination of the "trend" toward earlier physical maturation[26] suggests that the fertility of affluent Western-type societies might be explained partly in terms of changes in fecundity rates, especially when it is remembered that restrictions on opportunity to copulate have become relaxed in these societies. A failure to practice effective birth control is more likely to lead to pregnancy for a fecund than for a sub-fecund couple, and the variations in fecundity in this latter respect might affect as much as one third of the population.[27]

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Clearly, the sociologists' main contribution to the knowledge of the factors underlying fertility will be in their examination of the social influences on the opportunity to copulate and on the effective use of birth control. Within marriage, the former of these has largely been ignored, possibly because evidence from the 1930's has since been confirmed—namely, that coital frequencies vary with the age of wife and not with class or religious differences among populations.[28] In societies or in sections of societies where family relationships are unstable, however, the absence of one party for relatively long periods of time might affect the fertility of *married* couples. For example, tables in the 1911 Family Census show that officers in the Navy, Marines, or the Army with wives married at ages twenty to twenty-four had smaller completed families than those of any other upper-middle-class groups except barristers (that is, for marriages contracted between 1851 and 1880).[29] Unfortunately, instability of relationships may not necessarily reduce total exposure to risk. As Judith Blake has put it in her discussion of the Jamaican conjugal union: "On the one hand, it may reduce the rate of reproduction by decreasing the total amount of exposure to pregnancy. On the other hand, it may increase the motivation to reproduce because, with each new union, there is often a desire to have children born of that particular union."[30] It cannot be argued, therefore, that particular social situations which segregate the spouses more than is customary for their age group generally will *necessarily* result in smaller families among this minority. Nevertheless, the general point still stands that only survey research is likely to provide information on attitudes toward childbearing and their influence on coital frequency. Records from the past on the latter in particular are scanty, to put it mildly.

The historical sociologist can make a contribution to the study of the major influences on opportunity to copulate as regulated by nuptiality, especially in respect to fluctuations over time. Two kinds of statistics are available here: the proportions of men and women in a society who never marry, and the average age at marriage of those who do. Not much, it must be confessed, is known directly about the first of these, other than that marked variations exist.[31] By implication, the same social forces which produce later marriages result in a larger proportion of the never married, although at first sight there appears to be a clear distinction between what Malthus called "moral" restraint ("abstinence from marriage, either for a time or permanently, from prudential considerations"[32]) and

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celibacy, accepted for ascetic and religious reasons. If it is argued that the characteristically "European" pattern of marriage is not a simple product of urban industrialism but is *sui generis*,[33] and that sexual asceticism has always been a high ideal for European Christians (however much at times celibacy itself has been found to be impractical even for the clergy[34]), prudential and ascetic restraint might be seen as part of a single *Weltanschauung* derived from the Christian ethic. Within the same general body of doctrine, the future time perspective of prudential restraint, which is this-worldly, might in this sense be linked to another future time perspective, one which sacrifices the pleasures of this world for the beatitudes of the next. The purely historical part of the exercise then becomes that of showing, along lines which have already been made famous by Max Weber, how, if at all, the former developed out of the latter.

To be genuinely sociological, this would require the research to be conducted against the framework of a number of hypotheses, open to modification as the work proceeds. Such hypotheses might be:

1. that postponement of marriage, both absolutely and temporarily, results from its evaluation in terms of a future time perspective—that, for example, those people who refrain from marriage at any point in time demonstrate that they have done so as a result of a time perspective which also influences other decisions they make;

2. that within the body of Christian doctrine current at this point in time the future time perspective on worldly matters is positively associated with conceptions of a favorable destiny after death—that, for example, those people who hold a future time perspective express themselves on it in terms of Christian eschatology;

3. that, over time, discussions of Christian doctrine show a shift in emphasis from asceticism to prudence as realizable worldly means to other worldly ends—that is, that more of those Christians who wrote about these matters in an earlier period stressed the former, whereas more of those who wrote later stressed the latter.

The assumption here is that those writings which have come

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down to us are, so to speak, representative of the views of the doctrinaires of their time. It should also be noted, moreover, that in practice the historical sociologist will not be able to take a statistically random sample of those who postponed marriage. On the contrary, he is more likely to take examples of people whose biographies and so forth demonstrate that they possessed or did not possess the necessary future time perspective and to see whether they did or did not postpone marriage in terms of it.[35] Nor will the historical sociologist always know what Christian doctrine explicitly meant to these people, although he might make some kind of inference from religious adherence, provided that there was evidence of the strength of an individual's religious convictions and of the eschatological doctrines of the body of believers of which he or she was a member. The sociologist using historical data must be what Kitson Clark has called a "critical" historian, but at the same time he must never cease to be a sociologist.

Against this may be set the view that survey research on couples at the present time is the more rewarding method of tackling such questions as the relationship between the nature and strength of a religious conviction and "abstinence" from marriage, total or temporary. Unfortunately, historical sociology must make regular use of indirect evidence—"that is, evidence left by men and women who were intent on their own business and had no intention of leaving behind the information which is being used"; men and women who, when they were writing it down, "were unaware of the ghostly figure of the historian standing beside them and therefore did not modify their words to impress him, or lie to deceive him."[36] Many sociologists would regard such indirect evidence with some suspicion, especially if it included impressions and assertions about people other than the author. Indeed, sociologists often mistrust historical data because they have found that "many popular impressions about social relations in our own time are incorrect." They have, in consequence, "become sceptical of the possibility of ascertaining the facts about historical populations whose members can no longer be interviewed or observed."[37] Such sceptics fail to see, perhaps, that historical data of one sort, such as a Victorian factory inspector's views on the effect of factory employment on early marriages in cotton areas, may be checked against census and other data of the same period to arrive at an impression of the degree of bias shown by any commentator.[38] Moreover, in spite of statistical and other devices to overcome the

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impact of interviewer variability and the influences characteristic of any situation of human interaction,[39] there is still no effective answer to the charge that direct evidence obtained from social surveys, especially on such emotional topics as family planning or religious dogma, is likely to be colored by attempts to impress or to deceive the interviewer. Indeed, the kind of care which a historian takes to check his sources by internal and external criteria is often lacking in survey research which omits to check its results against what has been recorded elsewhere in documents.

Thus, there seem to be disadvantages in both methods of testing hypotheses. The weaknesses of the social survey in recording changes in attitudes and, more particularly, changes in situation over time, suggest that documentary and historical sociology are essential for the study of certain types of problem. A person's memories of what his past attitudes were on getting married or having a child are notoriously subject to modifications over time, and his present aspirations about future behavior are not necessarily the best guide to what he will actually do. Thus, a comparison between what engaged couples said was their preferred family size and the number of children they had about twenty years later has led to the conclusion that "the direct approach of asking people at marriage how many children they want leaves 90 per cent of the variation in total fertility unexplained"[40]—a percentage which increases to 96 when non-planners alone are considered, but still leaves 80 per cent unexplained when family planners are considered separately.[41] The events and circumstances influencing a couple's decision to marry or to have a first, or another, child are so manifold that only a successive series of interviews to ascertain changes not only in views, but also in economic circumstances and other aspects of their situation could possibly provide the kind of detail in depth which would be superior to *post hoc* documentary analysis, and even then only if those conducting the research had had sufficient foresight, or luck, to hit upon exactly the correct questions to ask.

This is not meant to imply that the documentary or historical approach to the study of marriage and fertility is, on balance, superior to conducting surveys. Both methods are required, although it may well be that different sociologists will have to conduct them because the kinds of demand they make call for different temperaments or personalities. The study of family planning, using effective birth control, is surely a case in point. Contraception has, of course, been employed throughout human history. What is particularly

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striking about the present era are three factors related to "the decision to conceive" that were absent during earlier periods: the rationalization of techniques—that is, their employment in terms of a logical or even scientific cause-effect sequence rather than in terms of magic or folk beliefs; the widespread dissemination of contraceptive information; and the psychological availability, or willingness, to use contraception—a willingness supported by moral, aesthetic, and related attitudes.[42] Clearly, only a survey could supply data on whether or not a couple uses birth-control techniques and why. It might also provide information on how the couple has come to learn about birth-control techniques and whether they regard them as effective—information which may be correlated with the degree of effective family planning achieved. On the other side of the picture, however, the *sine qua non* would seem to be how the information is disseminated throughout a community and how religious, political, and moral teachings on population are propagated—surveys like the Cornell University International Population Program's content analysis of newspaper clippings related to population from countries of America other than the U.S.A. and Canada.[43]

Such analysis, it might be objected, still utilizes contemporary material. A more precise study of the demographic transition in the countries of its origin would, however, require the application of these techniques, as well as the traditionally historiographical ones, to documents which have survived for a hundred years or more, especially since it is reasonably clear that the process of dissemination throughout South America, Africa, and Asia is only the second stage in a democratization process,[44] which began in Western Europe and the U.S.A. At the same time, the techniques used in the content analysis of contemporary newspapers might well be used with profit by historical sociologists who have been concerned with the spread of family limitation in the nineteenth century. A coding system, similar to the one used at Cornell, would also seem to be necessary if the 12,500 newspapers and periodicals issued in England between 1824 and 1900 and offering some 25 to 50 million articles[45] are to be examined systematically for the changing patterns of attitudes toward marriage and the family at a crucial stage in the history of population trends. The continuous interchange between methods of work, documentary and survey, applied to the testing of the same hypotheses, but against the background of different initial conditions is the most fruitful approach

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to the study of population. The regular reassessment of the results of this work, focused upon a relatively small number of explanatory propositions along the lines at present being pursued by James Beshers, would seem to be the most promising and exciting at this time.

One final point is perhaps worth making. As Beshers himself has indicated, part of the justification for the sociological approach to the study of population is that it will help in the formulation of more effective policy at a time of population explosion. Many historians do not need to be told this for their discipline, because they already believe that a knowledge of history is an invaluable basis for tackling at least some aspects of contemporary social issues. Sociologists, however, need to be convinced that the use of historical data can be illuminating for this purpose. From an examination of the part played by the English feminist movement in fertility decline in the nineteenth century, my wife and I concluded that the important factor was the attitude of the Victorian male toward family planning and that the decision to use, or not to use, birth control was generally not within the power of wives to make. We thereupon suggested that for societies *at the present time,* in which major decisions are a male responsibility, it is a waste of effort to direct birth-control propaganda at married women.[46] At about the same time, the very same point was being made independently by J. M. Stycos, from his knowledge of surveys conducted in non-Western societies.[47] Thus, historical and contemporary research may be seen to supplement each other in arriving at a policy decision—namely, that to be effective propaganda must be directed at husbands rather than wives, wherever their dominance demands it. From our point of view, this is a good example of the fundamental unity of the sociological approach, irrespective of the techniques of research used. When both historical and contemporary data confirm the same proposition, confidence in its essential accuracy is enhanced.

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T. H. HOLLINGSWORTH

The Importance of the Quality of the Data in Historical Demography

THE POPULATION of Africa, south of the Sahara, in the seventeenth century B.C. may be regarded as an obscure enough field for statistical research, and yet we can state the birth rate and the death rate that must have prevailed to some apparent degree of accuracy: namely, to within a factor of two. Taking 28 per thousand per year as our estimate for each of these vital rates, the correct values would only have to lie between 14 per thousand and 56 per thousand. Hardly any populations have ever been found with vital rates outside these limits, and even then they are only just outside them, so the assertion seems justified, although scarcely worth making. Methods of estimating the total size of this same African population are also necessarily vague. Some broad considerations about the level of culture, or assumed culture, may result in a figure of, say, five million people. The degree of uncertainty, however, is much greater than it is in estimating the birth and death rates. To apply to this area the highest and lowest densities of population ever found elsewhere would be absurd, and the resulting range would be very great indeed. On no reasonable assumptions, however, can the range of reasonable likelihood for such an obscure population as that of Africa south of the Sahara for the seventeenth century B.C. be made as short as a factor of two either way. A factor of five (that is, between one million and twenty-five million) would not be too small. Even for the seventeenth century A.D., we can scarcely do much better for this particular part of the world. Dire famines and droughts may have restricted population to a handful; long-forgotten cultures may, on the other hand, have thrived in their way, but left no artifacts because they happened to use im-

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permanent materials. We do not know what the population was, and probably we shall never know it except within very broad limits.

An important conclusion can be drawn from these considerations —namely, that different kinds of statistics require different orders of accuracy. Ascertaining a birth rate (or its more refined equivalent) to within a factor of two is a pointless exercise; ascertaining the population of an area or of a city to within a factor of two is more worthwhile. Correspondingly, an uncertainty of 20 per cent in a birth rate owing to shortcomings in the data is more serious than an uncertainty of 50 per cent in a total population size from the same causes. All demographic statistics are based on somewhat imperfect data, and it is important to bear in mind the different values that these statistics have. Ordinarily, the more refined the index of demographic behavior, the higher the quality we demand of the data. In this essay, we shall consider some examples of historical demography in terms of the quality of the data and suggest what can be done and what cannot.

Categories of Error

The meaning of terms needs to be clarified first. Error, bias, uncertainty, and mistake are all used to describe similar phenomena. It is useful to be clear in what ways they may be said to differ.

The fundamental point is that our philosophy of statistics must be somewhat more sophisticated than the simple idea that a figure means what it says. There are four respects in which a figure may not be simply what it seems to be: first, random error, caused by sampling; second, the intrinsic bias of the observer who reported the figure; third, what might be called factual errors; and fourth, mistakes. The first is the metaphysical basis of standard statistical practice, and given this basis, modern statistical techniques follow by fairly simple mathematics. By logical steps, we can produce significance tests, confidence intervals, frequency distributions, and the rest of the statistician's panoply. In expositions of these techniques, the mathematics is usually emphasized at the expense of the philosophy, although there is sometimes a difficult introductory section devoted to philosophy which it is easy to leave unread, since it generally seems to have little connection with the rest.

The first respect in which a figure is not quite what it appears to be is not, however, the only kind of error that a statistician can surmount. A limited amount of observer bias also presents no more

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than a routine difficulty. The meaning of observer bias may be explained by an example. If an observation were made to the effect that there were thirteen baptisms in a certain parish in a certain year, we should know that exactly thirteen in one particular year has no great significance because random error causes some annual variation in the number. The thirteen baptisms might, however, have been a figure provided for us by an assistant, and liable to some observer error that was specific to him. He might have zealously copied all the baptisms out of the register, for example, ignoring references to some of them as having been performed by the parish priest, but actually occurring outside the parish. The observer in question would thus have achieved an upward bias in his figures. It is not difficult to imagine other such faulty manners of observation that could possibly occur, all of which would cause irremovable errors if the registers were no longer extant. In normal statistical practice (in doing a sample survey, for instance) such observer bias is discovered only after all the data have been collected and can rarely be traced to its ultimate cause; one only knows that A gets figures that run 3 per cent too high for consistency with the other observers whereas B is 2 per cent too low. Adjustments are then made, empirically, to the data during the process of analysis. This type of error, however, is rare in demographic work.

The third category, factual errors, is worth most attention, because data containing factual errors can be grossly misleading; it is often possible to avoid some of the error by one special argument or another. By factual errors we mean misinterpretations of the data. In extreme logic, factual errors cannot, of course, occur; one could always talk of "recorded baptisms," for example, and never of "baptisms." The jump of inference between the two is essential to the inquiry, however, and not to make it would leave us with barren mathematics only. Yet there is reason to believe that the jump may not be the skip over an imaginary line we hope it is, but a leap over an immense chasm of doubt. Almost always, the trouble is not any consistent bias of the particular observer, but is caused by factors beyond his control. Generally (in fairly detailed statistics) there is a certain amount of omitted data, caused by the limitations of the observer's interests, time, and energy. It is possible, but not likely, that a vain inflation of records may have occurred; but a record-keeper is more likely to be lazy than vain. A chronicler who had no real records at all to work from, on the other hand,

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would no doubt have used his imagination and would be more likely to have exaggerated his numbers than to have diminished them.

Finally, by mistakes we mean errors in copying, in arithmetic, or in logic, and also any deliberately promulgated lies that the student makes the mistake of swallowing. The category is familiar to historians already, and little more need be said about it here.

History and Demography

Because contemporary demography is based upon statistics collected from the existing population, the data can be improved by a second enumeration or a sample check. Historical demography, on the other hand, deals with unique sources which can never be improved. Really reliable population figures will never be available for most people and periods, and the historian who hopes to base his argument upon a firm foundation must hope in vain. Other historical evidence will have to suggest, in part at least, what the true population levels and trends were, with the consequent risk of a circular argument. This means that more care must be given than heretofore to the degree of strength of a set of population data, and an assessment must be made of what deductions they will bear.

Professional historians seem to have acquired an unfortunate prejudice, when dealing with numbers, that small numbers are "safer" in some sense than large ones. At the time of Montesquieu's *Lettres Persanes*[1] (1721), exaggeration was still common; since Hume's essay *Of the Populousness of Antient Nations*[2] (1752), historians' estimates of population in the more distant past have often been minimum estimates only. When working chiefly from such reports as the numbers killed in a battle or living in a city, historians are fully justified; and in the matter of total numbers, since all the figures for the period would be deflated to much the same degree, it cannot be especially serious. When birth rates are estimated in this way, however, we can easily suppose that births were being limited, and when infant mortality is investigated, we may get absurd rates. Birth rates below the high levels found today in many parts of the world are, *a priori,* unlikely; infant mortality rates under 150 per thousand are scarcely credible. It would be most useful to attempt not only to determine the lowest conceivable rates, but also to determine the highest conceivable vital rates that could have obtained, assuming conditions favorable to such high levels. This would lead to the extent of undercounting that would be

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required to produce the observations, or rather to eliminate all the nonobservations. One would then have to consider whether such undercounting might possibly have occurred.

The origin of this attitude would seem to be the conviction with which Hume demolished Montesquieu's earlier view. Two samples of Montesquieu's prose will give some indication of the state of historical demography in Europe at the time[3]: *"La Grèce est si déserte, qu'elle ne contient pas la centième partie de ses anciens habitans. L'Espagne, autrefois si remplie, ne fait voir aujourd'hui que des campagnes inhabitées; et la France n'est rien, en comparaison de cette ancienne Gaule dont parlé César."* ("Greece is so deserted that it does not contain one hundredth of its ancient inhabitants. Spain, once so full, today only displays uninhabited country; and France is nothing in comparison to the old Gaul of which Caesar speaks.") He concludes: *"Après un calcul aussi exact qu'il peut l'être dans ces sortes de choses, j'ai trouvé qu'il y a, à peine, sur la terre la dixième partie des hommes qui y étaient dans les anciens temps."* ("After a calculation as exact as can be made on this sort of thing, I have found that there are scarcely one tenth as many people on earth as there were in ancient times.") The emphasis of the *Lettres Persanes* was, in fact, not on history, but on the contemporary world. It was a work of satire, and the *"calcul aussi exact qu'il peut l'être"* was based on no more solid evidence than sweeping generalizations, like those for Greece, Spain, and France quoted above. No one reads *Gulliver's Travels* for geographical information, nor supposes that all the lands it describes really existed, merely because Swift employs some verisimilitude in his satire (such as having Gulliver visit Japan among many fictitious places).

It was easy, therefore, for Hume to refute Montesquieu convincingly enough to impress the world of learning. He produced evidence that some of the ancient figures for population must be wrong since they are contradictory, and that many others are highly unlikely. Two points he made in his essay are, however, more relevant to the present growth of work in historical demography. He says:

> The facts, deliver'd by antient authors, are either so uncertain or so imperfect as to afford us nothing decisive in this matter. How indeed cou'd it be otherwise? The very facts, which we must oppose to them, in computing the greatness of modern states, are far from being either certain or compleat. . . . 'Tis to be remark'd, that all kinds of numbers

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are uncertain in antient manuscripts, and have been subject to much greater corruptions than any other part of the text; and that for an obvious reason. Any alteration, in other places, commonly affects the sense or grammar, and is more readily perceiv'd by the reader and transcriber.[4]

A proper result of Hume's work would thus have been to spread doubt about the possibility of being very informative about historical populations. It was one thing to argue that Montesquieu's 90 per cent reduction in world population over seventeen centuries was wrong, but another to say what the true change had been. Hume thought that world population in his own time was probably higher than at any time in the past; he clearly knew that this was only probable, not provably certain.

Critics of Historical Demographic Data

In most statistical work, the data are implicitly assumed to be without factual errors or mistakes. Statisticians are no different from historians in their preference for observations that mean what they say. Some theory has been developed that suggests when outlying values should be disregarded, but even this did not receive much consideration until the computer became a practical tool for data processing. An elaborate theory deals with random errors, but factual errors receive slight attention. Even the theory of random errors is comparatively new; seventy years ago, astronomy and surveying alone used a theory of errors, essentially based on Gauss's method of least squares. The economist and the historian, where they used numerical data at all, were bound to work only with large numbers so as to avoid risk of random error, and they had to assume that their data were either good and therefore usable or bad and therefore useless. The effect was to inhibit the study of historical demography. John Rickman, who was responsible for the first four British censuses, asked for information on the baptisms, marriages, and burials by parish over the preceding century, and eventually back to 1570. But he clearly realized that the quality of the returns was very poor, and he analyzed them only in a rough way, never publishing the original figures at all.[5]

According to E. Biot, the first student of historical populations who paid serious attention to the reliability of the data was not David Hume nor John Graunt, but Ma Tuan-Lin, who lived in the thirteenth century.[6] The section of Ma's *Wen-hsien t'ung-k'ao*

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(Literary Encyclopaedia) entitled *Hu k'ou men* (Doors and Mouths) deals with population statistics for China from earliest times to Ma's own day, ending in the year 1223. Biot, and S. Wells Williams[7] also, had a great respect for this Chinese scholar, who was fully alive to the errors that readily enter population statistics, especially when they are not derived from a census conducted expressly for the purpose of ascertaining the population, but are by-products of some other inquiry carried out for taxation or military purposes. Questions of completeness of coverage particularly concerned Ma.

A brilliant essay by Goran Ohlin has recently criticized three particular pieces of demographic research, considering whether it is really possible to derive any significant results from them.[8] The three topics he considers are somewhat diverse, which makes Ohlin's essay especially rewarding since it gives a variety of insight into the problems confronting the historical demographer. They are mentioned briefly here to give some indication of the diverse skills that historical demography may require and the small result that may be all that can be achieved from a laborious study.

First, Ohlin draws attention to the size of the samples from which J. C. Russell calculated, twenty years ago, his Life Tables for Medieval England. At some child ages, Russell had only single figure numbers at risk, making any calculation of Black Death mortality, for example, liable to wide error—independent of any errors in the actual data or of possible non-representativeness of the sample population.

Second, Ohlin appraises Karl Lamprecht's work, eighty years ago, on the number of villages in the Mosel Valley between the years 800 and 1237. The main argument here is that we cannot know the average size of these villages, which may well have changed greatly over as long a period as 437 years. Apart from correcting a misunderstanding of Lamprecht's figures, Ohlin points out that counting a list of place names is not quite the same as counting all the villages in an area, so that even if we knew the average size of a village, we should still be uncertain of the true population of the area.

Third, the difficulty of converting records of death taxes that were paid into death rates or even into relative death rates is explored in Ohlin's essay. His data were the heriots (a heriot was a tax payable upon a tenant's death) paid between 1246 and 1348

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by tenants on five manors belonging to the Bishop of Winchester, and he shows that the sharp increase in the number of heriots paid after 1289 might indeed mean higher mortality, but it could alternatively mean that properties had become subdivided so that there were more tenants and, therefore, more deaths.

Among the many other serious studies of the reliability of miscellaneous data relevant to historical research, only Louis Henry's articles on funerary inscriptions[9] can be noticed here. They should be known to anyone who is interested in the possibility of calculating mortality rates in the past from a series of ages at death as recorded. He showed, for instance, that whereas more than 40 per cent of all deaths in France between about 1812 and 1834 were before age fifteen, for a particular cemetary at Lyons less than 10 per cent of deaths with an inscription were before age fifteen. Henry thus demonstrated that only a general indication of adult mortality in the past can be gleaned from funerary inscriptions, owing to the selectivity exercised in deciding who will be given a memorial. Skeletons, the main source of demographic data for prehistoric times, must also have their limitations.

Parish Registers

The Anglican parish registers, on which demographers largely rely for data on vital rates in England before civil registration began in 1836, have been studied by J. T. Krause.[10] He estimated that 10 per cent of births and 5 per cent of deaths were omitted around 1700, and that after 1780 a deterioration set in resulting in near collapse of the system between 1795 and 1820. Among the causes of this decline in coverage, Krause cited the growth of nonconformity, the costs of burials, and the diverse ways in which the clergy obeyed the original order of 1538 that had established the parish registers. It is also noteworthy that London and the other cities about which we have most information have lower levels of completeness than Krause's guesses of 90 per cent for births and 95 per cent for deaths. In this connection, D. V. Glass[11] has recently shown that parish registers in London at the end of the seventeenth century could well have undercounted births by a very serious amount, at least 20 out of 83 known births in two parishes having been missed in the registers. Although it is possibly reasonable to expect the cities to be worse than the rest of the

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country because of more dissent and a more transient population, the better communications and higher standard of living of the cities might be expected to work for a better keeping of parish registers. The poor coverage that is known to apply to London can hardly encourage one to expect good coverage elsewhere.

Parish registers, in fact, are the chief source of data for recent research in historical demography. For the clergyman, however, keeping a parish register may have been like keeping his diary. He remembered to write the event down at once perhaps 80 per cent of the time; sometimes he forgot and never wrote it up afterwards. There was no proper check on him, and his level of enthusiasm must have been high to collect as much information as he normally did. Keeping a register *in vacuo*, so to speak, can have been no labor of love for many of the clergy. Moreover, it is easy to assume that most children were baptized, but how do we know whether it was 90 or 99 per cent? Every village in England is traditionally supposed to have had one squire, one idiot, and one atheist, for example, and these people might have been regarded as being respectively above, beneath, and beyond the church. We certainly know of areas of downright dissent, but what of areas of grudging assent? The conscientious parent who had all his children baptized may well have been the rule, but will not have been the universal rule. For telling us roughly how large a parish was, the registers are thus adequate; questions arise only when we want to study the birth rates and death rates.

A thorough method of exploiting parish registers was first put forward by M. Fleury and Louis Henry in 1956 and has been very widely used since.[12] It cannot be improved upon, but we must consider how likely it is that the data are good enough for it to be properly applicable. If they are not good enough, nothing can be done except to omit parts of the analysis.[13] General comparisons of the level of fertility and age at marriage are less likely to be spoiled by poor data than are the more intricate calculations, such as birth-interval analysis or mortality during the first year of life. The small number of families living in a parish means that sampling errors, too, must be avoided by taking rather large time-periods—during which, the practice of keeping the parish register could have changed.

Some studies using the Fleury-Henry method of ransacking parish registers and family reconstitution have arrived at suspiciously low rates of infant mortality, if we bear in mind that (allowing for

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probable under-enumeration) 200 or 250 per thousand infants apparently died in their first year of life among the high aristocracy until the eighteenth century. Thus E. A. Wrigley,[14] in what is otherwise an excellent study, found 1,480 baptisms[15] at Colyton between 1600 and 1649, but only found 138 buried in their first year of life. At such a place and time we might expect about 300 infant deaths for 1,480 births, so that unless Colyton were exceptionally healthy, it would mean that extra infants should be added who were neither baptized nor buried, owing to early death. It would be necessary to raise the Colyton baptisms by about 200 (more than 13 per cent) to get the expected rate of infant mortality, and it would mean that only some 40 per cent of the infant deaths were actually recorded. Wrigley's breakdown of infant mortality into seven groups by age suggested to him that at most 6 per cent of the baptisms might have been under-registered, and he compared them with figures for the parish of St. Michael le Belfrey in York during the years 1571-86.

An alternative comparison with Crulai[16] is interesting. In that French parish, we have data for two periods, from 1688 to 1719 and 1720 and beyond—that is, until about 1750. The under-registration of mortality was estimated at between 3 per cent and 12 per cent for the later period and was assumed to hold for the earlier period also. These are minimum estimates, moreover, in the sense that they suppose that registration of burials was virtually complete at Crulai. Baptisms were also assumed correct, so that, in fact, the correction is for migration only. For the earlier period, the infant mortality rate (uncorrected) was 236 per 1,000 (196 out of 829 born) and for the later period 172 per 1,000 (135 out of 786 born). Under-registration of deaths would raise these rates to 243-264 and 177-192, respectively, not too inconsistent with the figures for the British aristocracy, bearing in mind that the second period is well into the eighteenth century.

Wrigley, however, has a crude infant mortality rate for Colyton of only 93 per 1,000 (138 deaths out of 1,480 births). If under-registration of baptisms were at his maximum of 6 per cent, then 95 infants would have been missed completely, and the Colyton infant mortality rate for 1600-1649 would become 148 per 1,000 (233 out of 1,575). This figure is still well below Crulai's level a century later, itself low rather than high in relation to the British aristocracy.

Let us consider the division of deaths in the first year of life

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into those aged under three months and those older than that. It is convenient to take a fairly late point of division, so that difficulties over the considerable delay in baptizing newborn children that occurred in England but not in France have little importance. Mortality is much lower at the age of three months than earlier, and it will not matter greatly what the precise ages were within a week or two. On general grounds, we must expect the ratio of deaths under three months to deaths over three months to vary with the general level of infant mortality: the higher the infant mortality, the smaller the proportion of deaths in the first three months. We may verify this for Crulai. For the period 1688-1719, the ratio of deaths under three months to deaths between three and twelve months was 158 to 38 (4.158 to 1), and for the period 1720 and beyond, which had a lower infant mortality rate, the same ratio of deaths was 118 to 17 (6.935 to 1). At Colyton, the uncorrected deaths numbered 90 before the age of three months and 48 between three months and twelve. Adding 95 infant deaths, all very likely under three months, is not enough; the new ratio, 185 to 48, is still only 3.854 to 1, although the infant mortality remains less than either of the Crulai rates. The best fit of the Colyton figures to the Crulai ratios (linearly interpolating) would be to add a further 96 missed baptisms to Colyton, all assumed to correspond to deaths under the age of three months. We should then find an infant mortality rate of 197 per 1,000 (329 infant deaths out of 1,671 births) and a ratio of infant deaths under three months to infant deaths over three months of 281 to 48, or 5.854 to 1. Any under-enumeration at Crulai owing to migration would affect this calculation, and so the true infant mortality at Colyton is unlikely to have been below 200 per 1,000, as anticipated. Fortunately, Wrigley's conclusions about fertility changes are modified, but not contraverted, by such a high rate of omission.

Genealogical Research

Ideally, we need a way of appraising data that does not lead us into supposing that all the observations we find are all the observations that an ideal observer could possibly have found. This, of course, will require more care than if we had no such worries, since we must accord at least some passing thought to the reliability of each observation. No observation can be discarded, and few or none elevated to the special position of being thought indisputable. If,

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for example, a man said his age was forty in response to an inquiry, then that is what the datum is, even if his true age was forty-six, and he later said that it was. From this it follows that a comprehensive set of data must include both estimates of his age, or at least pay some attention to both of them. Although forty-six would be correct, only an ideal observer would know that it was correct, and we should be forced to pay some attention to the figure of forty. The analysis would have to include a criterion for deciding what to do about the two figures. Four possibilities exist: accept forty (because it came to us first), accept forty-six (because we believe it is the better of the two figures), accept both, or discard the data as intractable. The third is the most difficult to handle, but the most rewarding in the end. It means that not only will our results be liable to sampling error and our hypotheses confirmed or contradicted at a certain level of probability, but also that an extra error term will be incorporated into the analysis that will indicate how far any conclusions at all can be drawn.

The collection of data under these conditions is, of course, more arduous than when data are gathered in the ordinary way. It is, however, a regrettable fact of a statistician's experience that bias creeps in readily whenever a sample is not truly random, but has selected itself in some way. For this reason, some method of using the vital events of every member of a population is much to be desired, and in two studies on the British aristocracy[17] such a device has been elaborated. This method would be applicable to any study based on genealogies, although it does not suit parish register work.

Very widely varying degrees of information exist about the dates of historical events. For many events we know the exact day, and from some Chinese genealogies, where astrological interest has preserved the information, the exact hour of a birth is known.[18] In genealogical studies at least, there is then a second group for which the dates are a little uncertain—we may know only the date of burial rather than the day of death, or we have two dates between which a birth or death occurred; marriage licenses granted on a certain day may point to a marriage soon afterwards, and so on. There is next a third group—those people whose age is known to the nearest year at some point, but whose date of birth seems now to be lost. For example: a man's age may be mentioned in the proof of a will; he may have stated his age in a dated letter to a friend; or his portrait may give his age at the time it was painted. There is

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a fourth group, which needs care owing to contradictory evidence which sometimes occurs, comprising cases where more accuracy can be achieved making a birthdate fit in with those of the siblings, whose relative order of birth is almost always known, at least within each sex. There remains, however, a considerable fifth group of people who are poorly documented, for whom no direct indication of age exists. Yet general considerations often show, within five or ten years, limits within which they must have been born. For example, the date a man joined the army or when he entered an Inn of Court as a student may be known. Similarly, dates of death are often known to within twenty years or less from documents that mention a person as being alive or dead.

The principle used in the larger and later study on the British aristocracy to classify the raw information according to its degree of accuracy was that of the binary logarithmic scale.[19] A range of one year either way was taken as the center, and other classifications went from one thirty-second of a year up to thirty-two years either way. There were thus eleven classes, the shortest of which was all time-ranges up to three weeks; the twelfth class consisted of a large group of dates assumed to be accurate to the day. It was then possible to include as much of the universe as seemed sensible for the different parts of the study, and to study the bad data each time to see whether or not they suggested a bias. Neither would have been possible if all data below a certain level of reliability had simply been discarded. In actual fact, a certain amount of absurdity came into the estimates of age at death because all those with the same type of uncertainty had been placed at the same age. Adult married females with no further information about them, for instance, were always made to die at age fifty-three—a figure arbitrarily derived from Indian life-tables and the usual age at marriage as found in the first study.[20] It would have been possible, and better, to distribute these cases stochastically over the range of ages suitable to them; but should further study of the raw material bring to light some of the vital dates for these people, there is at least the comfort that no bias would be expected when correcting the tentative dates.

If it be allowed that the general quality of information about dates of birth must be connected with complete omission of an individual, it becomes possible to derive estimates of the rate of omission. The method assumes that there was a constant sex ratio at birth, but that boys and girls were not equally likely to be left out.

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Almost all the omitted people probably died young, particularly in the first month or so of life. This method—the only way of deducing the omission rate that has so far been suggested, unless two independent enumerations happen to have been made of the same population—seems much better than just assuming that the results "look good." The levels of omission found ran from about 16 per cent of the true births for those born around 1600 to about 10 per cent around 1700, about 2 per cent around 1800, and less than 1 per cent around 1900.[21]

The child death rates were profoundly affected by these omissions, a point overlooked in a recent study in which the ratio of the corrected figures for the cohort of both sexes born between 1575 and 1599 was treated as though it had been directly observed.[22] There is no reason to claim more for such an early cohort than that female mortality was probably lower than male mortality (which was not true in the next century) and to estimate roughly how much lower it was. Calculating the female advantage for different ages is meaningless, in view of the irremovable errors in the data.

It is depressing to think that a large proportion, perhaps a sixth, of the people who ought to have been under scrutiny were phantoms, leaving no trace of their birth or death and not so much as a name to remember them by. Yet it is not incredible, and one must not pretend that complete records are common. In China, extensive population records have been kept for centuries, even for millennia. Chronic errors crept in, however, so that most of these records are probably badly wrong, with an occasional jump when they were belatedly improved.[23] It is less likely that gross lying occurred at the time of the jump than that some long-established errors were removed and the earlier figures had been far from the truth. A valuable corrective to undue belief in the completeness of historical vital statistics lies also in the study of more recent demographic data. Errors of the grossest sort occur in many registration systems today. In India, for example, the registered births suggest that the national birthrate is about 25 per thousand; examination of the census and sample inquiries, however, leads to a figure of about 45 per thousand. Even in Victorian England, some 6 per cent of births went unregistered before 1860.[24]

The Future

There has been some demand for a more detailed analysis of

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the social patterns of the British aristocracy following the description of their demographic levels and trends.[25] It is only prudent, when considering what could be done, to bear the limitations of the data in mind. There is no doubt that eldest sons of peers led different lives socially than their younger brothers and that the latter often did not marry or else married late. Some study of these social differences would be interesting and would not be affected by the omission rate. The heir, on the one hand, was under pressure to continue the line and produce a large family; on the other, he usually married so young that estrangement from his wife after a few years of marriage was not rare. On balance, the eldest sons probably had the higher level of fertility, adjusting for age at marriage, but a proper study has still to be made.

The most remarkable thing to a demographer that studies of the aristocracy have shown, however, is that fertility among the children of British peers fell steadily between about 1620 and 1740. At the beginning, the fertility of this group was so high that the population was tending to grow at a rate equivalent to about 2 per cent per annum; by the end, it had fallen so far (helped by later marriage and higher mortality rates under age fifty) that a net decline of population of about ½ per cent per annum is indicated by the generation replacement rate. The subsequent changes are more familiar and easier to explain: the rise in fertility until 1830 and its steady fall since, flattening out in the twentieth century; the steady fall in mortality; the high age at marriage until the end of the nineteenth century, and its subsequent collapse. But the fall in fertility between the reigns of Charles I and George I—the main factor in the halving of the rate of replacement per generation—is beyond adequate historical explanation at present, although the French nobility behaved in much the same way during this period.[26] (They did not, however, share in the rise in fertility after 1740.) It will not do to bring in external causes, such as plague, and economic factors surely had little effect on a population whose mean income must have been many times greater than the national average. Social pressure is the probable explanation, but what kind of pressure? There was a major political revolution in the 1640's which affected the social standing of the aristocrats, but it seems very odd that their fertility should have declined for a whole century afterwards as a result. The Restoration, after all, was in 1660, not 1740. Alternatively, if the "general tenor of aristocratic life" is invoked as being anti-fertility for some reason, why

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did the British aristocracy reverse its fertility trend about 1740 and the French aristocracy continue theirs?

Further research would do well to concentrate on the period 1620-1740 and consider which groups of families led the downward trend. Distinctions by national group (English, Scottish, or Irish), Whig or Tory, Catholic or Protestant, high- or low-ranking peers, old or new families, and (to some extent) rich or poor are obtainable in principle and might be relevant to the investigation.

Extensions at both ends of the time-scale would also be practicable. At the later end, peers dying after 1938 were excluded, which was convenient for technical reasons. Since peers' children were the main universe studied, the numbers of those studied shrinks for all generations born after 1875. At the earlier end, although the records are poor, some attempt might be made to gather data back to 1283, when the House of Lords was constituted in something like its present form, for the number of people involved is relatively small. Here detailed consideration of the accuracy of all estimates of dates would become very important, since few dates beyond all question are available and otherwise the work cannot be done.

Historical demography is a difficult subject. The collection of data is laborious, requiring checking and a watch for hints of underenumeration. The analysis is often subtle, since errors in the data need to be assessed. The conclusions may seem too trivial to be worth so much effort. Yet the historical demographer's aim is to produce the best conclusions that can be drawn from the extant material. Scholarship that tries to do more must be vain.

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JOSEPH J. SPENGLER

Demographic Factors and Early Modern Economic Development

> How slender an initial difference may come to be decisive of the outcome in case circumstances give this initial difference a cumulative effect.
>
> —Thorsten Veblen

SOMETIME BEFORE the seventeenth century, perhaps as early as the fifteenth, a unique marriage pattern began to develop in Western Europe. It apparently did not exist in the fourteenth century, but had become effective by the seventeenth, if not earlier. John Hajnal has described the genesis of this pattern, which seems to have been a product of both the rational elements in Western European culture and the prevailing institutional arrangements associated with that culture.

This pattern consisted of much later marriage than one found in Eastern Europe or outside Europe. It significantly intensified the birth-limiting influence of ecclesiastical and other forms of celibacy. It contributed greatly to the relatively low level of European birth rates which, "so far as we can tell, were rarely over 38 before the spread of birth control." Thus, it impeded the rate of population growth. This slowing down augmented, in turn, per-capita productivity as well as capital formation and cushioned the impact of numbers upon land and resources. It made average income higher than it otherwise would have been and thereby facilitated the emergence and successful launching of the Industrial Revolution, which finally freed Western man, especially the common man, from the trammels of static, catastrophe-prone, and poverty-ridden pain economies.[1]

Population and Income Growth

The course of both population and income growth corresponded

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rather closely to that of a Malthusian model in Europe until around the sixteenth century, and in most of Asia and Africa almost to the present. Europe's population did not begin to grow continuously and without marked interruptions until the fifteenth century, but thereafter it proceeded at a slowly increasing rate. Europe's population, M. K. Bennett suggests, increased by about three quarters between the years 1000 and 1300, only to decline by perhaps three eighths during the next century, and then to return to the 1300 level by the early-sixteenth century. Thereafter it grew—about one quarter of 1 per cent per year in the sixteenth and seventeenth centuries and somewhat more rapidly in the eighteenth century, especially in the latter part. The long-term annual growth rate averaged about one half of 1 per cent in the eighteenth century and about three quarters of 1 per cent in the nineteenth: This increase in the growth rate seems to have been attributable mainly, if not entirely, to a decline in mortality. Before the nineteenth century, this decline is traceable chiefly to a marked diminution in the incidence of famine and pestilence and the catastrophic mortality associated therewith, especially in such periods as 1349-1470's, aptly described by Sylvia Thrupp as "the golden age of bacteria."

The movement of the per-capita output of goods and services in Europe was long dominated by the course of its population growth, itself often under the empire of unfavorable events and therefore very low over the long run according to modern standards. The countries of Europe were predominantly agricultural, with three quarters or more of the population being rural. The mode of agricultural production was relatively traditional and unchanging. Supply was quite inelastic, and its increase depended principally upon the extension of cultivation. English and other data suggest that productivity was low and progressed slowly even when population pressure encouraged the abandonment of the three-field system. In Western Europe, as Phyllis Deane and W. A. Cole write: "The significant variable in the long preindustrial secular swings in productivity seems to have been the rate of population growth. When population rose, product per head fell: when population fell, product per head rose." The Malthusian tendency for numbers to keep abreast of agricultural output seems to have been characteristic also of Asia. Despite a generally higher birth rate, the population of Asia supposedly increased only about five sixths between 1650 and 1800, no more than that of Europe and Russia. Mortality regulated population growth in Asia much more than it did in Europe.

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As K. W. Taylor concluded: "Long periods of [population] stability are best explained by the Malthusian hypothesis, and the relatively short periods of rapid growth can be explained in terms of major technological or environmental changes." Even distribution of population, A. P. Usher found, was closely associated with the availability of food. This correlation remained constant until the Industrial Revolution converted mineral resources into a major determinant of population location. Density varied, of course, reflecting in part the fact that full maturity of settlement was not achieved in Northwestern Europe until around 1600—sixteen centuries later than in the Mediterranean world, but three centuries earlier than in India and China. In Northern and Western Europe, density varied greatly even before the Industrial Revolution, ranging from 137 inhabitants per square mile in Belgium and parts of Italy through 90 in England and 110 in France to about 5 in Norway.[2]

So long as the numbers of people tended to press closely upon the food supply, average income and output could not increase greatly over the long run. The elasticity in the supply of produce was too low. Average output increased with the extension of settlement, the formation of capital, and the stimulus of the temporary economic upsurges manifest after 1100 and again after the late 1400's, but not much of this gain could be retained. Indeed, if it had been, average income in 1500 might have exceeded subsistence by 300 to 400 per cent, rather than the actual, much lower, margin. If average output in the Roman Empire around A.D. 200 had approximated 1.25 times a hypothetical subsistence level and thereafter had grown about one tenth of 1 per cent per year, by 1500 the level of output should have been about 3.5 times as high as it was thirteen centuries earlier, but it was not.

As English data suggest, in and long after the Middle Ages, population pressure tended to develop in the wake of a continuing growth in numbers. It became manifest, for example in the late-thirteenth and early-fourteenth centuries—after several centuries of growth, much of which was subsequently wiped out by the Black Death. Population pressure was again felt in the sixteenth and seventeenth centuries and resulted in complaints of unemployment as well as calls for the development of colonial outlets to absorb the excess numbers.[3]

Even so, as Deane and Cole state, England and Wales had apparently achieved by the late-seventeenth century a higher level

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of material welfare than any other country with the exception of Holland, whose average income, according to Gregory King, was slightly above the English average and at least one fourth above the French average. Deane and Cole add, however, that the seventeenth-century English average did not greatly exceed that of the "rest of the world" or that of fifteenth-century England. They also observe that "change, outside the cataclysms produced by famines and epidemics, was generally small, slow, and easily reversed" in the seventeenth-century world in which King lived.[4]

Three observations may now be made. Although our information in respect to Europe's rate of population growth before 1800 is incomplete, this rate appears to have been low, averaging perhaps one quarter of 1 per cent per year between 1500 and 1700 and about five twelfths of 1 per cent per year between 1700 and 1750. In France population increased somewhat between 1500 and 1600, though not steadily; in the seventeenth-century, Pierre Goubert believes, it oscillated around an equilibrium of about nineteen million, standing nearer the minimum than the maximum in 1700. It grew about one third of 1 per cent per year in the eighteenth century, about .22 per cent per year in 1700-55, and about one half of 1 per cent in 1755-1801. In England and Wales, population grew about one third of 1 per cent per year between 1500 and 1700 and between 1700 and 1780. Even if English incomes and wages were not much greater in the late-seventeenth century than in the late-fifteenth, the slowness with which the population grew at least permitted the standards attained in the late-fifteenth century to be retained and perhaps even improved. Given the low rate of natural increase, over-all real output per head could increase something like .3 per cent annually in England and Wales between 1700 and the 1770's, even though the aggregate real output grew slowly throughout the eighteenth century.

Despite the slowness with which average output advanced, it rose above the levels encountered in much of today's underdeveloped world. Phyllis Deane estimates that the average income in England and Wales in 1750 (at least one third above the 1700 level, but about two fifths below the 1800 level) would have been equivalent to £70 in the 1950's—a figure nearly treble that of India and two thirds that of Mexico in the 1950's. According to Simon Kuznets, "per worker income in the agricultural sector in today's most populous" underdeveloped countries (China, India, Pakistan, Burma, South Korea, and most of Africa) is only "one

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fourth or one third of per worker income in the currently developed countries in their preindustrialization phase" when six tenths or more of their labor force remained in agriculture. Accordingly, *total* income per worker in these countries was two to three times as high as it is in today's underdeveloped countries. "By comparison with many present-day preindustrial economies in Africa and Asia," Cole and Deane conclude, "the English economy of the late seventeenth and early eighteenth centuries had reached a relatively advanced stage of economic organization. So too, no doubt, had certain other countries."[5]

Theories of Population and Income Growth

A low rate of population growth may encourage the rate of growth of average income in three ways. These observations are especially true of predominantly agricultural economies that are equipped with an essentially traditional agricultural technique, are not yet subject to economies of scale, and are just beginning to assume a more commerical-industrial form.

If a population is growing slowly, the bulk of the savings which it generates can be devoted to increasing capital per head, thereby easing indirectly the pressure of population upon land and facilitating the growth of nonagricultural enterprises. Capital formation proceeded at a very slow pace in the seventeenth and early-eighteenth centuries. In England around 1700 the annual rate of saving fell within a range of something like 3 to 6 per cent of the national income. The French rate was probably lower, and the Dutch rate may have been higher. With such low saving rates, capital per head could increase significantly only if the rate of population growth were negligible and savings were not employed wastefully, as often happened in ancient and medieval times. Were population growth negligible and were only 4 per cent saved and invested annually to yield a 10 per cent rate of return, average income would rise about .4 of 1 per cent per year, enough to increase per-capita income by approximately one half in about ninety-five years. Were population growing as much as 1 per cent per year, this growth would absorb virtually all the savings, should they amount to only 4 or 5 per cent of the national income. Not even in Europe, however, was the former option always exercised. According to E. F. Heckscher, population growth in eighteenth-century Sweden absorbed the disposable resources available.[6]

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Slowing down population growth decreases the impact numbers and agriculturalists have upon cultivatable land. Accordingly, the ratio of the agricultural labor force to cultivatable land was only 10 to 50 per cent as high in pre-industrial European countries as it is in today's underdeveloped countries. Even so, in and before David Ricardo's day, there was strong evidence of a classical diminishing of returns in England and possibly elsewhere. Nevertheless, because numbers had grown slowly, and limits to production had not been reached, time remained during which yields per acre could be increased or manufactures could be developed and exchanged for imports. This time was utilized, moreover, and yields were increased somewhat, certainly in greater measure than they have been today in those underdeveloped countries whose agricultural productivity has increased little over the centuries.[7]

Potential productivity per capita in a population depends markedly upon the size of the fraction of the population that is of working age. Let this group be represented by those fifteen to fifty-nine or fifteen to sixty-four years of age. This fraction varies appreciably. In 1965, for example, the fraction aged fifteen to sixty-four approximated 54 per cent in Africa, Latin America, and Southeast Asia, much less than the 64 per cent found in Europe. Therefore, under *ceteris paribus* conditions, potential productivity was about 1.185 (64/54) times as high in Europe as in Africa, Latin America, and Southeast Asia. Northern and Western Europe enjoyed superiority in this respect as early as 1850, when the fraction aged fifteen to sixty-four years ranged between 60 and 66 per cent. Of course, the increase in potential productivity made possible would be actualized only if there were not offsetting increases in unemployment.

Before we attempt to estimate the magnitude of this fraction in the seventeenth and eighteenth centuries, we may examine a hypothetical stable population to observe how an age structure is generated by past mortality and fertility patterns. The relative number of persons of working age in a stable population rises as fertility (measured by the Gross Reproduction Rate [GRR]) and life expectancy at birth decrease; the relative number falls as fertility and life expectancy rise. If, for example, we hold expectation of life at birth constant at fifty years, the proportion aged fifteen to fifty-nine rises from 45.8 per cent with GRR at 4.0 to around 60 per cent with GRR at 1.25 to 1.50. If we hold GRR constant at 2.0, the proportion falls from 64 per cent with an expectation of

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life at birth of twenty years to 54.7 per cent with a life expectancy of over seventy years (70.2). Thus, declining mortality cancels out a portion of the improvement in age structure associated with a decline in the GRR. For example, combining a life expectancy of thirty years with a GRR of 3 results in a fraction of 0.545 aged fifteen to fifty-nine; a life expectancy of 70.2 with a GRR of 1.5, in a fraction of 0.577 aged fifteen to fifty-nine.

Information on the age composition of the pre-industrial countries of Northern and Western Europe may be found from two sources. The first source, hypothetical stable populations, furnishes only inferential information. In the body of Table 1, I give the percentages aged fifteen to sixty-four in stable populations resulting from combinations of GRR (see column 1) with expectations of life at birth (see line 2 in head of table). This fraction descends from 64 per cent when a GRR of 2.25 is combined with a life expectancy of twenty-five to fifty-five when a GRR of 3.0 is combined with a life expectancy of forty. The numbers in parentheses are rough indicators of the rates of natural increase per 1,000 inhabitants associated with designated combinations of GRR and life expectancy. We see at once how the proportion aged fifteen to sixty-four declines as GRR and life expectancy at birth increase.[8]

Table 1

Proportions Aged 15–64 Years: Rates of Growth

	Expectation of Life at Birth			
GRR	25	30	35	40
2.25	64(−3)	62(3)	61(7)	60(11)
2.5	62(1)	61(7)	59(11)	58(15)
3.0	59(7)	57(13)	56(18)	55(22)

The problem confronting us is that of selecting those combinations in the table which most closely approximate fertility and mortality conditions in pre-industrial Western Europe. The rate of population growth must be low. Life expectancy probably did not exceed thirty-five years in whole populations before 1800. Let us suppose the most representative combinations to be those associated with a GRR of 2.25 to 2.5 and a life expectancy of thirty to thirty-five years. We would then expect the proportion aged fifteen to sixty-four to approach 59 to 62 per cent or, more likely, 61 to 62 per cent, since the rate of population growth was decidedly

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below 1 per cent per year. This proportion is at least 10 to 15 per cent greater than levels encountered in many of today's underdeveloped countries.

Extant data relating to fertility and mortality in pre-industrial Europe presumably support the suppositions made in the preceding paragraph. First, the birth rate was much lower in pre-industrial Europe than it is in present-day and pre-1900 Asia and Africa. During the 1700's the rate was around thirty-five, somewhat lower in England and Wales, France, and Scandinavia (exclusive of Finland, which had a rate sometimes just over forty). Between 1776 and 1800, when the Swedish birth rate averaged somewhat below thirty-five, the GRR averaged about 2.1. We find birth rates of roughly thirty-five to thirty-seven associated with a GRR of 2.25 and of roughly thirty-eight to forty-one associated with a GRR of 2.5. Although the birth rates reported for eighteenth-century Europe may be somewhat off the mark, they do suggest that the GRR was close to 2.25 or 2.5.

So far, information on mortality and life expectancy before 1800 is limited. Around 1800, expectation of life at birth may have been as high as thirty-five to forty years in some Western European countries; if so, it was higher than it was during the sixteenth and seventeenth centuries. Life expectancy at birth rose in Sweden between the period 1755-76 and the period 1816-40, increasing from 33.2 to 39.5 years for males and from 35.7 to 43.5 years for females. Female life expectancy at birth, usually somewhat higher than male life expectancy, was 44.7 years in Denmark in 1835-44; 42.18 in England and Wales in 1841; 40.83 in France in 1817-31; 37.91 in Iceland in 1850-60; and 35.12 in the Netherlands in 1816-25. K. F. Helleiner suggests that life expectancy rose during the eighteenth century because "the periodic erosion of growth by epidemic or harvest failure became less marked," especially after 1750. An alternative might be that other changes gave rise to a decline in mortality during this century. Mortality, however, appears to have been greater in the earlier parts of the eighteenth century and in the seventeenth century. Gregory King's data suggest a life expectancy of only thirty-two in England during 1690's, a figure which Peter Laslett finds fitting "in fairly well with what is known for the seventeenth century both in England and France." Studies of localized populations in the first half of the eighteenth century in France yield an estimate of about thirty-three years for expectation of life at birth. According to S. Peller, life expectancy at birth

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among Europe's ruling families averaged only about thirty-one years in 1600-99, three years less than in the preceding century and six years less than in the eighteenth. Generation life tables of the British peerage suggest that expectation of life at birth was thirty-three years among males born between 1680 and 1729, but almost forty-five years (44.8) among those born between 1730 and 1779. If violent deaths are not included, male life expectancy at birth for those born between 1330 and 1679 remained in the range of thirty or thirty-one years. One may infer from these diverse data that life expectancy at birth probably did not exceed thirty-five years in any country before the late-eighteenth century.

Our second source of information is the statistics available for the age composition of pre-industrial Europe as well as Japan, a country whose people controlled their numbers. Persons aged fifteen to sixty-four constituted 60.5 and 62 per cent, respectively, of Sweden's population in 1750 and 1800; 62.2 and 61.4 per cent of France's population in 1775 and 1801; and 58.4 and 60.8 per cent of Japan's population in the eighth century and 1888. The proportion of the Danish population aged fifteen to sixty-four in 1787 was similar to that in Sweden in 1750. Iceland's population in 1703 included a larger proportion aged fifteen to sixty-four. Data on the age composition of eighteenth-century Europe suggest, when contrasted with those in Table 2 discussed below, that this age composition was more favorable to economic production than that now found in much of the underdeveloped world with its high fertility and relatively low mortality. Although the conditions that made for high mortality and, hence, for a relatively low proportion of persons under fifteen years of age must have debilitated the population in some degree, they could hardly have offset the advantages derived from a low rate of population growth.[9]

Thus, the age composition of populations in Western European countries was favorable to average productivity. It is difficult, of course, to assess the change in age composition that took place after the adoption of the unique Western European pattern of marriage. This may have increased the favorableness of the age composition by at least three units, or about 5 per cent. If per-capita productivity rose correspondingly, the rate of saving could have increased by a larger fraction. In any case, until late in the eighteenth century, the slow rate of population growth permitted savings to be devoted almost entirely to improving productive capital per head. This slowness also held down the rate of increase

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in population pressure, perhaps sufficiently to permit improvement in yields to offset small increases in population.

Yesterday's Europe Versus Today's Africa and Asia

Today the economic distance separating various parts of the underdeveloped world from various parts of the developed world is many times what it was two to three centuries ago. At one time, this distance might have been easily bridged. Had, as late as 1800, the index of performance in the most advanced lands been four times that in the least advanced, and had this index continued to improve at a rate of 1.5 per cent per year in the more advanced countries, the laggard countries might have caught up by the early-twentieth century. This did not happen. The index rose most rapidly in the advanced countries. Today real private consumption per head is between six and thirty-one times as high in Northern and Western Europe as it is in various underdeveloped countries; it is even higher in the United States. This increase in spread is the result of what was done in the European sphere and not done in the underdeveloped world. In the latter category must be included failure to curtail fertility.

Agricultural population density increased faster in Asia than in Europe. By 1948, it was nearly double that in Africa and Europe, exclusive of the Soviet Union (147 per square kilometer of available land in Asia as contrasted with 83 and 86 in Africa and Europe).[10] Over three fifths of the labor force of Africa and Asia remained in agriculture, although output per agriculturalist and per acre remained low, often too low, to supply minimum nutritional requirements.

Population grew faster during the nineteenth century in Europe than it did in the underdeveloped countries, with their high levels of mortality. This relationship was eventually altered. Mortality declined in European countries, but so did the birth rates. Today the rate of population growth is much higher in the underdeveloped world than elsewhere and is a number of times what it was in pre-1800 Western Europe. Most Gross Reproduction Rates in Africa, mainland Asia, and Latin America fall within a range of 2.5 to 3.5; expectation of life at birth falls below forty years in Africa, between forty and fifty years in Asia, and between fifty and fifty-five in Latin America. The rate of natural increase per decade between 1970 and 2000 is expected to range from around 20 per cent in

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Asia to around 30 per cent in South America and, perhaps, Africa. These prospective rates, roughly double what they were in the period 1920-40, reflect declines in crude mortality uncompensated by declines in natality. These high rates of population growth will absorb much of the capital that is formed in the underdeveloped world and hence will greatly retard growth of output per head.

The age composition of the present-day underdeveloped countries is much less favorable to productivity than that encountered in the developed world or in European countries during their pre-industrial stage. As of 1965, the fraction formed by the population fifteen to sixty-four years was as follows: East Asia (exclusive of Japan), 52.5 per cent; South Asia, 54.7 per cent; Africa, 54.2 per cent, ranging from 51.7 per cent in West Africa to 56.1 per cent in Eastern Africa; Central America, 50.8 per cent; Tropical South America, 53.4 per cent; Caribbean America, 56 per cent. The Philippines is exemplary, for the current fraction, 53 per cent, is much below the 57.2 per cent reported for 1903, but is consistent with an estimated birth rate in the upper-forties and a death rate in the low-twenties.

These percentages are compatible with current estimates of gross reproduction (2.5 to 3.0 in Africa and 2.5 to 3.5 in Asia and Latin America) and life expectancy at birth (around or below forty years in Africa; forty to fifty in Asia; fifty to fifty-five in Latin America). In Table 2, GRR and life expectancies are combined to yield stable populations and the percentages of the population aged fifteen to sixty-four. (In parentheses, we give the approximate rates of natural increase associated with these combinations.) The data suggest that in Africa the fraction aged fifteen to sixty-four will range between 55 and 58 per cent; in Asia, between 50 and 58 per cent; and in Latin America, between 49 and 56 per cent.

Table 2

Proportions Aged 15–64 Years: Rates of Growth

	Expectation of Life at Birth		
GRR	40	50	55
2.5	58(15)	56(22)	56(23)
3.0	55(22)	53(28)	52(31)
3.5	52(27)	50(34)	49(37)

The relatively low rates of fertility and natural increase—in

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part the result of the unique fertility-regulating pattern of marriage that developed in Europe during the fifteenth or sixteenth centuries—must have contributed greatly to the growth of average income in the seventeenth and eighteenth centuries. The sustained increase in average income did not, of course, give rise to the Industrial Revolution, for this revolution, unlike the minor ones of the twelfth, fifteenth, and seventeenth centuries, was a *revolutionary* transformation of the British and Western European economies. It was the product of many cooperating factors (capital accumulation, inventions, innovations, favorable factor endowments, *laissez faire,* market expansion, earlier experience, and so forth). Among its causes was the relatively low level of fertility existing in Western Europe. Because fertility was low, the relative number of persons of productive age and, hence, the potential productivity per head were high. Natural increase also was low, so that the pressure of population upon agricultural land did not build up rapidly and nearly all savings could be devoted to improving the stock of capital instead of supporting population growth. Average income could rise, albeit slowly and somewhat intermittently, and pave the way for the coming of the Industrial Revolution.

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DAVID M. HEER

Economic Development and the Fertility Transition

POPULATION THEORISTS have developed contrasting views concerning the effect that economic development, or increase in per-capita income, has on fertility. One school contends that economic development inhibits fertility. This view, perhaps predominant in recent years, is expressed most succinctly in the theory of demographic transition set forth by Warren S. Thompson, C. P. Blacker, Kingsley Davis, Frank Notestein, and others.[1] According to this theory, a nation's demographic process depends on its stage of industrialization and, by implication, its level of economic development. Warren Thompson and Kingsley Davis, for example, divide the nations of the world into three classes. Class I nations are highly industrialized, have low fertility and mortality, and show little or no population growth; Class II nations are beginning the process of industrialization, have declining although still high fertility, rapidly declining mortality, but, in net balance, a high rate of population growth; Class III nations are not yet industrialized, have both high fertility and high mortality, and, at most, only moderate population growth. The theory of demographic transition, popularized around the time of World War II, is congruent with the generally inverse association between fertility level and degree of industrialization among nations today. Indeed, fertility levels of industrialized nations are lower than they were before the nations became industrialized.

A second school of thought, however, argues that economic development promotes fertility. Perhaps the foremost representative of this viewpoint was the English economist T. R. Malthus. He believed that an increase in the demand for labor increased the proportion of persons marrying and reduced the average age at marriage, and that this change in marriage pattern led in turn to

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an increase in fertility.[2] Because Malthus lived during the middle of the Industrial Revolution in England, his views were presumably colored by what he conceived to be recent trends in fertility in his own country. Later critics of Malthus, considering his views on the relation between fertility and economic development to be erroneous, generally attributed the large acceleration in population growth which accompanied the Industrial Revolution exclusively to lowered mortality. Several historical demographers have, however, recently produced evidence indicating that fertility may well have increased during England's period of industrial development in the early-nineteenth century,[3] and that similar increases may have occurred in parts of the Netherlands.[4]

Support for the school of thought linking fertility increase to economic development has also been supplied by studies indicating a relationship between birth and marriage rates and business cycles.[5] Virginia Galbraith and Dorothy Thomas have demonstrated that business cycles in the United States between 1919 and 1937 affected both marriages and births. With an appropriate time lag, marriages and births of each parity increased when business conditions improved and declined when business fell off.[6] Dudley Kirk analyzed the situation in Germany during the 1920's and arrived at similar conclusions.[7] Researchers working with such data from other countries have in all cases shown a positive correlation between birth and marriage rates and the height of the business cycle.[8]

Evidence that economic development promotes fertility also comes from other types of studies. From his analysis of both the influence of general business conditions on fertility and the effects of the relative demand for labor on young persons of reproductive age, Richard Easterlin hypothesizes that the high wage and salary levels of persons twenty to twenty-nine years old was one of the prime factors sustaining the American baby boom of the 1950's. The number of persons in this age group was exceptionally low because of the small number of babies born during the Depression. The supply of new entrants to the labor force was, therefore, abnormally reduced during the 1950's, a period when demand for labor was high. Moreover, the group entering the labor force in the 1950's was exceptionally well educated in comparison with older age groups and thus had a competitive advantage in employment during a period in which educational qualifications became increasingly important.[9]

A recent study by W. Stys further supports the conclusion that

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a high economic level increases fertility. Stys has shown that for Polish peasant women born during the latter half of the nineteenth century, average completed family size varied directly with size of farm. Among landless peasants, the average number of births per mother was 3.9; on farms of more than seven hectares, the average number was 9.1. The difference in fertility by size of farm results mostly from variations in the mother's age at marriage. There was, nevertheless, some tendency for births per year of marriage to be slightly higher among women living on the larger farms.[10]

The results of surveys conducted by Gordon De Jong imply that respondents of each social class would increase their fertility as their economic circumstances improved and would reduce it were their economic circumstances to decline. De Jong asked respondents in the Southern Appalachian region of the United States what they considered to be the ideal number of children for the average young couple today, for a "well-off" young couple, and for a "not well-off" couple. The respondents in the study believed that the ideal number of children for the "not well-off" couple would be 1.5; for the average couple, 2.79; and for the "well-off" couple, 4.02. Similar results were obtained separately for respondents of each social class.[11]

Deborah Freedman has been concerned with the extent to which the husband's "relative income" affects fertility of individual married couples in the United States.[12] (By "relative income" she means the degree to which the husband's actual income exceeded or was exceeded by that of men in his socio-economic reference group—that is, men of similar age, occupation, income, and region of residence.) Her analysis was made with data from a national probability sample of fecund, white, nonfarm wives eighteen to thirty-nine years old married at least five years. After controls for thirteen other relevant variables, she found that the number of children born to the wife was positively related to the magnitude of the husband's relative income. After ten years of marriage, wives whose husband's relative income was low had .42 less children than would have been expected on the basis of the other variables; conversely, wives whose husband's relative income was high had .21 more children than would have been expected.

The two contrasting views concerning the effect of economic development on fertility obviously demand reconciliation. I have elsewhere hypothesized that economic development directly increases fertility, although various other factors usually accompany-

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ing the process of economic development serve to reduce fertility.[13] The indirect effects of economic development tending to reduce fertility are often, albeit not invariably, stronger than the direct effect tending to raise it. Thus, economic development frequently results in fertility decline.[14] Making use of data for forty-one nations during the 1950's, I found that the average level of national fertility was inversely associated with per-capita net national product when no other variables were held constant. The relation between the two variables was slightly positive, however, after instituting controls for other relevant variables—the level of infant mortality, per-capita newspaper circulation, population density, and the recent percentage increase in per-capita energy consumption. Controlling for these other variables, I found that per-capita newspaper circulation and population density were inversely related to fertility, and that infant mortality and recent increase in per-capita energy consumption were directly related. After control for other variables, infant mortality showed a stronger relation to fertility than any of the other four variables. Increase in educational attainment, for which per-capita newspaper circulation is a good index, and decline in infant and early childhood mortality are, of course, two of the most pervasive phenomena accompanying economic development.

An understanding of the biological factors which constrain fertility is necessary to any analysis of fertility change. Nevertheless, fertility cannot be fully explained unless voluntary decisions concerning future children are also considered. Biological factors clearly place an upper bound on the fertility of each individual. Below the limit of biologically maximum fertility (probably around twelve births for the average woman[15]), each individual is more or less free to choose how many babies to have. Excess fertility can always be avoided even though the price for doing so may be great. To consider two extreme examples, couples can refrain from sexual intercourse to avoid a further pregnancy, and the pregnant woman can decide to abort the fetus even at the risk of her own life.

Joseph Spengler has devised an excellent conceptual scheme for analyzing the factors affecting the decision to have children.[16] He considers the decision to have an additional child to be a function of three variables: the preference system, the price system, and income. Provided these terms are given a broader definition than they usually receive in economic literature, the three concepts pro-

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vide a complete classification of all factors that affect such a decision. The preference system simply describes the value a married couple places on an additional child relative to the value of goals they might otherwise achieve. The price system delineates the cost of an additional child relative to the cost of attaining other goals that might be achieved were the decision to have another child not made. Costs must be broadly defined to include not only monetary costs, but expenditures of time and effort. Income, too, must be broadly defined so that it encompasses monetary income as well as the total amount of time and energy available to a couple in their pursuit of possible goals. (Because the term *resources* fits the definition more closely than the term *income*, I shall henceforth refer to the former rather than the latter.) Given these definitions, the probability of deciding in favor of another child will vary directly with the relative value anticipated from that child, inversely with the predicted relative cost, and directly with the amount of resources foreseen as available for all goals.[17]

Spengler's scheme should prove to be useful in analyzing the long-term changes in fertility in the now developed countries during the last century or so and in demonstrating further the thesis that economic development directly enhances fertility, but has indirect effects that lead to fertility decline. Any explanation of the variables accounting for the long-term fertility change in the developed nations should, however, be postponed until the magnitude of this change has been described. The accompanying graph shows, for example, the change in the total fertility rate for white women in the United States between 1800 and 1965. The total fertility rate for a given year is a summary measure of fertility, which can best be described as the total number of children a woman would have were she to live through the entire reproductive period and bear children at the average rate for that time. As one can see from this graph, the decline in the total fertility rate from 1800 to 1965 has been very pronounced. The regularity of the decline, however, is distinctly marred by the dramatic fertility rise during the 1940's and 1950's. Since 1957, the trend of fertility decline has been renewed, and from 1964 to 1965 the United States experienced the sharpest annual percentage fertility decline in its entire history (almost 8 per cent).

It should not be supposed that the temporal pattern of fertility decline has been identical in all of the developed nations. The magnitude of the decline was greater in the United States than in

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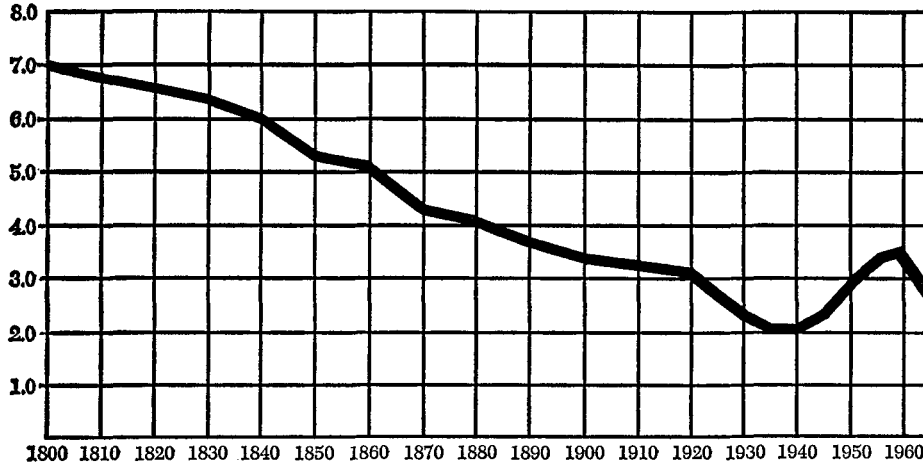
Figure 1

Total Fertility Rate for the White Population of the United States 1800-1965



Sources: Ansley J. Coale and Melvin Zelnik, *New Estimates of Fertility and Population in the United States* (Princeton, 1963), p. 36; U. S. National Center for Health Statistics, *Monthly Vital Statistics Report,* Vol. 15, No. 11 Supplement (February, 1967).

Europe. Furthermore, fertility reduction began much earlier in France and the United States than in Great Britain, where decline dates only from 1876,[18] even though England industrialized much earlier. Moreover, the baby boom following World War II was much more pronounced in the United States than it was in Europe.

The developed nations have a long history of increasing per-capita monetary income. According to Simon Kuznets, the average decennial rate of growth in per-capita national product in the United States between 1839 and 1960-62 was more than 17 per cent, a rate sufficient to increase per-capita product 4.9 times per century.[19] The developed nations also have a long history of decreasing hours devoted to gainful employment and increasing amounts of leisure time. Had there been no change in either the price or preference system, one might have expected that the long-term trend in fertility would have been upward. Since fertility has tended over the long run to go down rather than up, changes in the preference and price system must have discouraged rather than encouraged fertility to an extent that they counterbalanced the elevating effects of increased money and leisure time.

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On the other hand, the developed nations which did not suffer severely from World War II (United States, Canada, Australia, and New Zealand) underwent a substantial rise in fertility during the 1940's and 1950's. As I have noted earlier, Easterlin has provided extensive documentation that this period of rising fertility was also one of rapid rise in monetary income for young adults in the United States.[20] During the period of the baby boom in the United States, the amount of time, money, and effort available for child-rearing activities was markedly expanded by the increased willingness and ability of grandparents to help their married children in child-care responsibilities. The grandparents of the postwar baby crop had had relatively few children themselves and, therefore, probably welcomed the chance to share in the work of raising their grandchildren. Moreover, even though the number of children per parent was quite high during the 1950's, the number per grandparent was not large since the number in the parental generation was so small. Thus, grandparents could make a large contribution to the rearing of each grandchild in a way that will not, for example, be possible for the grandparents of the 1970's. We may therefore presume that during the period of the baby boom the elevating effects of rising resources more than counterbalanced any depressing effects of changes in the preference or price system.

In the last hundred years or so, several changes in the preference system of the developed nations have undoubtedly tended to reduce family size. One of the most important of these is the decline in mortality, which has of course been pronounced. In the United States for example, the mean expectation of life at birth increased from 47.3 years in 1900 to 70.2 years in 1964.[21] The secular (long-term) decline in mortality has had greater relative effect in infancy and childhood than adulthood. If fertility had not declined, the reduction in mortality would have tended to increase somewhat the number of living children per living parent. The United Nations estimates that for a population with high fertility (a Gross Reproduction Rate of 2.5) and very high mortality (life expectation at birth of twenty years), the ratio of population under fifteen years to that aged fifteen to fifty-nine years is 0.56. When the expectation of life at birth is increased to seventy years with no change in fertility, the ratio is increased to 0.83.[22] Thus one would expect the value of an additional birth to wane as the level of mortality declines.

There is also a possible connection between the level of mor-

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tality and the amount of emotional energy that parents invest in each of their children. It may be supposed that the pain of bereavement at a child's death is directly proportional to the amount of emotional energy that the parents have invested in the child. Where mortality levels are high, one might expect parents, in the interest of self-protection, to develop relatively little emotional involvement in any one child. A reduction in mortality encourages parents to place more libido in the existing children and thus should reduce their desire to have an additional child, since they have limited amounts of emotional energy.[23]

Lowering the mortality level should also reduce the desire for additional children because parents can be more certain of having a specified minimum number of children survive to maturity. When mortality is high, one cannot be sure that any of one's existing children will survive to maturity. When mortality is as low as it is in the developed nations, parents can be highly certain that their child will survive from birth to maturity. Thus, a decline in mortality reduces the value of an additional child as insurance against the possibility that one or more of the existing children may die. The effect of mortality reduction in this respect can be quantitatively measured. If one assumes that each couple is capable of bearing twelve children, that a perfect means of birth control is available and utilized, and that all couples want to be 95 per cent certain of having at least one son who will survive to the father's sixty-fifth birthday, the Gross Reproduction Rate will fall from 5.2 when the expectation of life at birth is twenty years to 0.95 when the expectation of life rises to seventy-four years.[24]

A second long-term change in the preference system relates to the value which parents can derive from the productive labor of their children. In the agrarian society of the United States in the eighteenth century, when the supply of land was practically unlimited, children could be productive assets to their parents at a very early age. As the amount of land per capita declined, as it did in the United States during the nineteenth century, the value to the farmer of the labor of an additional child probably declined correspondingly. In all of the developed nations, industrialization substantially reduced the value of child labor. Although such labor was quite common in many of the early factories, the situation of the child in the factory was much less satisfactory than it was when he worked under the direction of his father on the family farm. As a result, strong moral sentiment developed against child labor,

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and legislation restricting it emerged in all of the developed nations. In each nation, the development of this legislation was very gradual, and the early regulations were much less restrictive than later laws. The first such legislation in England, passed in 1817, merely banned children under nine years of age from working in cotton mills. In the United States, the first mildly restrictive legislation appeared in a few northern states about the middle of the nineteenth century.[25] Only in 1938 did the United States Federal Government enact child-labor regulations; these prohibited the employment of children under sixteen in manufacturing or mining and banned the employment of children under fourteen from all industry, except agriculture, which engaged in interstate commerce.[26]

The utility of child labor was further reduced by compulsory education laws which, as they increased in severity, also lowered the productive value to the parents of an additional child. Prussia, under the leadership of Frederick the Great, became in 1763 the first nation to legislate compulsory attendance at schools for all persons five to fourteen years of age. The first law establishing compulsory school attendance in England was enacted in 1876. In the United States, Massachusetts became in 1852 the first state to demand school attendance; similar legislation did not become universal until 1918, when attendance was finally made compulsory in Mississippi.[27]

The development of formal institutions to support the elderly has also brought about substantial changes in the value of an additional child. In the pre-industrial period and in the early stages of the Industrial Revolution, the elderly could expect to receive financial support only from their own kin—mainly from their sons. Gradually business corporations and governments developed social-security schemes for the aged and for widows. In the United Kingdom, legislation establishing old-age pensions for needy persons was first enacted in 1908, and in 1925 a contributory system covering all workers was established. In the United States, the first federal legislation concerning old-age, survivors', or disability pensions dates from 1935, although many private corporations provided pensions and insurance systems much earlier.[28] With the full development of social security, it became unnecessary for parents to bear children in sufficient number to assure that one or more sons would support them in their old age. Thus, the value to parents of additional children has been further diminished.

The preference system has also been altered by the decline in

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social rewards for bearing a large number of children. When mortality was high, a high rate of fertility was a positive necessity if the population was not to decline. Governmental and religious authorities who did not wish to see the nation's population reduced encouraged a high level of fertility. As mortality declined, however, a high level of fertility was no longer necessary to maintain the existing population level. As a result, many governments and religious bodies have shifted from a position favoring large families to one of neutrality or even opposition. A historic landmark was reached when the Church of England admitted at its Lambeth Conference of 1930 that mechanical or chemical means of contraception were not necessarily immoral.[29] Since that time, the Protestant churches have, for the most part, ceased to extol the virtue of large families, and in the present decade the Roman Catholic Church has been faced with a great internal struggle, as yet unresolved, concerning this question. Although no European government has become alarmed about problems of excess fertility—all, in fact, encourage large families through their programs of family allowances—the United States has for the first time, under the Johnson Administration, provided federal funds for the establishment of family-planning clinics, thus indicating that it no longer wishes to encourage large families, at least among the poor.

A fifth possible change in the preference system may be the result of a tendency for economic development to shift the criteria for social status from ascribed characteristics (such as birth into a particular family) to achievement. Where status is ascribed at birth, one need spend little effort in advertising one's status to others; where status is achieved, its level tends to be transitory, and individuals may develop an intense need for conspicuous consumption to demonstrate their rank. If the preference for conspicuous consumption increases, the preference for children, who do little to publicize one's status, should decline. J. A. Banks has provided extensive documentation to show that during the latter years of the Victorian Era, the British middle class felt an increasing need to engage in conspicuous consumption, and that English fertility first began its decline during this period.

The tremendous development of new and improved methods of birth control over the last century has not only reduced the relative preference for children, but has also increased their price relative to that of other goals. When available methods of birth control are crude and undeveloped, or knowledge of better methods is lacking,

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the decision not to have an additional child involves substantial inconvenience, interference with sexual pleasure, or even some hazard to health and life incurred by resort to a primitive means of abortion. Some of the major landmarks in the development of contraceptive technology during the last century were the manufacture of rubber condoms in the late-nineteenth century, the invention of a diaphragm by Mensinga in 1880, and in the 1930's the appearance of the latex condom, which was cheaper and better than its rubber predecessor.[30] Increasing use of the highly effective oral contraceptives in the United States and other nations during the 1960's reduced the penalties in deciding against another child and may have been one of the major reasons for sharp fertility decline. Although the "pill" was placed on the United States market only in 1960, by 1965 it was the most popular contraceptive, the method used most recently by 24 per cent of white wives eighteen to thirty-nine years of age.[31] Abortion has also been one of the principal means of birth control, and improvements in its technology have probably affected the preference for children. Little has been written about this, however, and it is difficult to ascertain the history of abortion techniques in those nations where the purposeful disruption of pregnancy is for the most part illegal.

Economic development has produced other changes in the price system affecting desired family size. Urbanization has been one of the most important concomitants of economic development. In the United States, the proportion of the population classified as urban increased from 5 to 70 per cent between 1790 and 1960.[32] In general, urbanization results in a rise in the relative price of living space. Since rearing of children demands considerable living space, the relative cost of children no doubt rises with each increase in the relative price of living space. Although the relative cost of living space has in general been increasing over the last hundred years, the rise has, perhaps, not been invariant. One may speculate that the increasingly widespread use of the automobile in the United States during the 1940's and 1950's, together with governmental policies which subsidized home-ownership, made possible the acquisition of suburban houses at a relative cost probably substantially lower than prevailed during previous decades. Part of the American baby boom of the 1940's and 1950's may be explained by this short-term change in the relative cost of living space.

The tendency for the labor cost of child care to rise relative to the labor cost of producing material goods no doubt is a factor

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affecting desired family size. While economic development makes possible a much larger production of factory goods per man-hour of labor, the number of man-hours necessary to supervise and socialize a child has certainly not declined and most probably has risen. When a married couple are deciding whether to have another child, they can assume that an additional child will burden the wife with the responsibilities of child care for about three years. Moreover, with another child to supervise, she will have to work harder during the period when the older children are still under her care. This increased effort must be set against the possible remuneration from a job. Since the amount of material goods which can be bought with each hour of labor outside the home has steadily increased with each advance in national economic level, there has been a substantial long-term increase in the price of child-care services relative to the price of material goods.

A final long-term change in the price system affecting the decision to have children concerns the quality of education which parents demand for their children and which is socially imposed. A society more and more oriented to a complex technology requires that children be given an increasingly lengthy education. Parents recognize that their own child will be at a substantial disadvantage unless his education meets society's new norm. Even where the direct cost of education is met by the state, longer education increases the cost to the parent in terms of more years of child dependency. Hence the secular rise in the standard of education has no doubt helped to depress family size.

The factors connected with industrial development which I have listed have, in my opinion, tended to depress fertility to such an extent that the actual trend has usually been downward despite the elevating effect increased resources have had on fertility. It is not yet possible to evaluate the importance of the role each of these factors has played in the temporal changes in fertility in the developed nations during the past century or more. Although their relative importance may never be well established, further historical research may be of great value. I would recommend, in particular, detailed study correlating fertility change with such matters as the development of social-security systems, the decline in the prevalence of child labor, increases in the proportion of children attending school, changes in the relative cost of living space, and augmentation of the relative labor cost of child care.

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Analysis of the past can be of some help in predicting the future course of fertility in both the more developed nations and in those currently less developed. For the economically advanced nations, certain of the factors which have operated in the past will in the future operate with much diminished force. It is, for example, impossible for infant and childhood mortality to drop much further. Again, provision for old-age security is now almost completely divorced from the extended kin-group, and little further shift from kin-group responsibility can be expected. Moreover, child labor has been practically eliminated, and additional change in this will be of no further importance to fertility. Thus, for the economically developed nations, at least three factors important in the past reduction of fertility will have little impact on the course of future fertility.

This might tempt one to predict that if per-capita income continues to rise in these nations, fertility will also rise. We should, however, be able to count on the continuation of certain trends which in the past have been inimical to high fertility. Most of these trends affect the price system. Since the ideal means of birth control has not yet been invented, we can anticipate some decline in fertility with each successive step toward this ideal. We can also predict that the mother's opportunity cost for spending time in child-rearing activities will continue to climb since the price of material goods will most probably continue to fall relative to that of providing child-care services. The demand for a higher level of education will in all likelihood also continue. Unless the cost of such education is completely socialized, this demand should constitute additional pressure for fertility decline. Barring unforeseen developments in transportation technology, increasing population density should make inevitable a further increase in the relative cost of living space, an important component of the total cost of rearing children. Finally, we can, I think, be fairly confident that public opinion will take an increasingly negative stance toward large family size. A withdrawal of social rewards for a large number of children might have a substantial effect on fertility preference. I believe that the public will be more and more aware of the taxes necessary to support population growth, of the ways in which a larger population aggravates urban congestion and crowds detract from the enjoyment of places of prime historic and scenic interest. If, however, the level of international tension is exacerbated, a contrary pressure in favor of larger families might ensue since each

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nation might fear that its power relative to other nations would diminish were its relative population size to fall.

The long-run fertility trend in the now industrialized nations will, I think, not be upward. Nevertheless, it is certainly plausible to assume that fertility may rise in several of these nations for certain short-run periods. For the less developed nations, it seems very probable that further progress in economic level should bring substantial fertility decline. In these nations, economic development should have all of the indirect effects that it had previously in Europe and North America. In many of these nations, there will also be governmental programs encouraging small families. Furthermore, the technology of birth control is now more advanced than it was when fertility first began to decline in the West; thus, the mere introduction of the new birth-control methods may bring moderate fertility reduction into populations only weakly motivated to reduce family size. The real question for the less developed nations is whether they will be able to attain a further measure of economic development. It is, of course, not impossible that in at least some of these nations population growth will outstrip the increase in the means of subsistence. If lower living standards ensue, the level of mortality may be greatly affected. Given a rise in infant and childhood mortality, it may then be impossible to obtain fertility decline.

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12. Deborah S. Freedman, "The Relation of Economic Status to Fertility," *American Economic Review,* Vol. 53, No. 3 (June, 1963), pp. 414-26.

13. See David M. Heer, "Economic Development and Fertility," *Demography,* Vol. 3, No. 2 (1966), pp. 423-44, where I set forth a theory which attempts to harmonize these contrasting views and offer empirical evidence of a new theory's validity.

14. Actually there are four possible versions of this hypothesis. In the first version, we refer to one national population over time and examine the effect of aggregate economic development and the aggregate factors accompanying it on aggregate trends in fertility. In the second, we look at the effect of the aggregate level of economic development and its accompanying factors on the aggregate level of fertility in a set of national populations at a particular time. In a third version, we look at the effect of economic development and its accompanying factors on an individual basis—individual change in income and style of life associated with that income—on individual fertility for a group of persons over time. In the fourth version, we examine the effect of the individual level of economic development and its accompanying factors on individual fertility for a group of persons at a given moment in time. Ansley Coale has correctly pointed out that a demonstration of the truth of one of these versions does not necessarily constitute proof of any other version. The results contained in my article "Economic Development and Fertility" are pertinent only to the second version, whereas my main interest in this paper is the first

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version of the hypothesis. Some of the other studies I have cited are pertinent to the third and fourth versions.

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17. I shall ignore the possibility that children are an "inferior good" so that as income rises children are substituted for more desirable objects which give the same sort of satisfaction only in higher degree. I do not consider this to be a realistic assumption.

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JOHN T. NOONAN, JR.

Intellectual and Demographic History

I

THE number of human births and deaths and the number of living human beings can be recorded, calculated, or estimated for various places and periods. To the historian, the tabulations made constitute data. They are pieces of evidence which he must question for their own built-in assumptions and then fit with the evidence offered by other disciplines from geography to theology in order to carry out his job of describing human action. This historical task necessarily will involve postulating motives, purposes, valuations, and rationality in the men whose actions are described. If the historian is writing about human action in relation to population, his job is to weigh the evidence and to estimate the elements of human response that are reflected in a particular birth rate or a particular growth. The rates of births and deaths are precious material for him, but the answers to his questions come only from an interrogation of all the evidence available.

Among the evidence, fragile, intangible, and nonquantifiable, are human ideas themselves. That ideas affect behavior is, I suppose, the assumption of everyone who writes history. At the very least, economic historians endow human beings with an economic rationality that leads men to match their behavior to their financial prospects. In the writing of demographic history, hypotheses as to the causality of economic calculations predominate, possibly because of the academic antecedents of demography and demographers. If a close correlation is found between births and some economic factor—the price of grain, the size of a lot, the index of industrial production—it is often thought that the reason for a change in births has been satisfactorily accounted for. It is often assumed that some kind of eco-

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nomic rationality determines human habits of breeding. Such economic rationality is itself a type of idea or system of ideas.

Demographers who think of their work with contemporary data as a science have difficulty understanding what a historian is interested in. They are used to proceeding on the principle of parsimony—Ockham's old principle that entities are not to be multiplied in causal explanations. In applying this principle they are apt to look first to a demographic factor—if the number of births goes down and the number of marriages is found also to have decreased, the latter phenomenon may be taken as the cause of the former. If the demographic factor is not found, they turn most readily to an economic explanation, sometimes with the unexpressed conviction that, at bottom, economic reasons underlie human actions. They seek to demonstrate their causal hypotheses quantitatively. They are accustomed to measurement and are content only if measurable relationships can be worked out.

When these assumptions, useful for prediction and control purposes in a laboratory, are transferred to history, there is danger of grave distortion. Historians do not believe in multiplying causes needlessly, but they are aware that in most persons and in most societies a multiplicity of interacting, reinforcing, counterchecking forces are at play. Parsimony is a poor principle if it suppresses part of the play.

Quantitative measurement, desirable if attainable, is not the only tool for understanding the acts of human beings. Ideas other than demographic observation or economic estimations are operative in the behavior of men. That such noneconomic ideas affect births and deaths as well as other human activities does not, *a priori*, seem unlikely. The extent and force of these ideas and their effect on population may be expected to vary with the ideas and with the people holding them. Competing ideas exist in the same society and in the same men, and to take ideas into account is not to measure phenomena, but to grasp a complex structure of interactions. In a complicated system such as Christianity, some ideas may promote population growth directly, others retard it directly, and others affect it indirectly. The particular groups accepting the ideas will emphasize different ideas. The ideas themselves will not constitute a constant, but will develop, reflecting the experience of those affected. One can scarcely speak of measurement. What is sought is insight. One does not seek a standard and a kind of proof irrelevant to the questions asked. To agree to omit the influence of noneco-

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nomic ideas in any history of human population because the calculation of their effect is not statistical must be regarded as an arbitrary cutting off of evidence, an inadequate depiction of human action.

Apart from the interaction of ideas with population history, demography itself depends on a number of theoretical assumptions. Any effort to view demography as a pure science of measurement necessarily neglects to take critically into account the notions of intellectual history which honeycomb demographic enterprises. What constitutes a village or a nation or the human race itself is a matter of theory. Unless one believes that a social, religious, political, or geographical grouping has theoretical significance, it is difficult to see why the figures of a given community are tabulated. The selection of what group will be studied is not a matter of tabulation, and yet this decision to choose, to isolate, to describe a particular slice of population is decisive for demography. Even in the fundamental task of tabulation, then, there is no pure demography of numbers; the data are always determined by particular purposes; a set of social, moral, political, and scientific interests frames the determinations made.

If this dependence of demography on theory exists in the examination of the births and deaths of human beings, it becomes accentuated when other categories are introduced as relevant. The simplest additional category is marriage. What is a marriage is determined by a legal, religious, or social standard, and the demographer may either take the standard used in the records given him or uniformly apply a social standard such as "a union lasting for a certain duration." In either case, he has to impose on the rough flow of human interactions a pattern which he deems significant for his purpose. By supposing that what he accepts as marriage has some relation to births, he necessarily ventures into the realm of causality, theory, and intellectual history.

II

The ideas most relevant to population in Western society are of a mythic-moral character. By ideas I mean beliefs, concepts, and valuations; by mythic I mean ideas, nondemonstrable but not necessarily untrue, of the nature of the cosmos in relation to the destiny of man; and by moral I mean prescriptions for human conduct in terms of a good. Of these mythic-moral ideas the most obviously

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relevant are those which assign a value to population. There is, for example, the belief advanced in the Babylonian Talmud that there are a certain number of souls waiting to be born, and that until they are born, the Messiah will not come.[1] There is the belief that procreation is necessary in order to perpetuate the race until the Messiah does come.[2] There are the thoughts, current since St. Augustine's time, that the population of heaven has been depleted by the fall of the angels and that this depopulation must be met by human efforts at procreation, "lest heaven be cheated of its number of citizens."[3] There is the belief of a minority of the Fathers that the Kingdom of God expands by procreation.[4] There is the belief, in countries divided by the Reformation, that the triumph of a faith depends on the birth rate of its followers.[5] In all of these views, population itself is a positive value.

Other mythic-moral views assign a negative value to population. There is the belief, current among Gnostic and orthodox Christians of the second century, that "the world is full": The optimum and maximum number of the redeemed exist; the Messiah has come; there is no need to continue the procreation of the race.[6] There is the even more severe view of the Manichees that the most sinful of deeds is the procreative act: It perpetuates in new human beings the imprisonment of the light particles, once parts of the Princes of Light, who should be liberated from the flesh to journey to the Father of Lights.[7]

A mythic-moral view need not be expressed in religious imagery. The ancient Greek view of procreation as an answer to death—the view that man lives in his descendants—had no such explicit imagery[8]; this belief is doubtless held by many nonreligious persons today. Modern valuations of population sometimes depend on the belief that procreation either does or does not serve "the nation," taken as an ultimate reality: Nazi Germany and postwar Japan are obvious alternative examples. Some contemporary American thought reflects the belief, common to almost all utopian literature, that a static population is best suited to human needs because it is so amenable to rational planning; in this thought, an ideal society on earth performs the function of setting population standards that Augustine once attributed to the population of heaven.

Relevant ideas need not contain direct statements on the value of population or the ends of procreation. Of almost equal immediacy to population size are ideas on marriage and sexual behavior, on celibacy, chastity, virginity, and widowhood. The special ideas of

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patristic Christianity, for example, form a cluster here. Celibacy as such is not valued, but marriage is not presented as the optimum state; chaste celibacy is preferred.[9] Widowhood is assigned a high value.[10] Marriage is presented as the sole framework in which sexual intercourse may occur.[11] Within marriage, value is given to sexual continence, even to perpetual continence.[12] All of these valuations, if put into practice, affect the likelihood of opportunities for procreative acts. They may be contrasted in their probable effects with the Old Testament ethic where marriage is the norm, virginity is to be kept only as a condition of a girl prior to marriage, there is no word for bachelor, and widowhood is diminished by the levirate law.[13]

The ideal content of marriage is also of substantial relevance. Consider the Christian goals of offspring, fidelity, and indissolubility in marriage. That monogamy alone is permissible, that slave concubinage to produce an heir may not be practiced, that divorce cannot be had for sterility are formal tenets of the Latin Christian Fathers.[14] These beliefs seem to reduce the occasions for procreation and, thus, affect the growth of population. On the other hand, the classic Christian authors of the West insist that the purpose of marriage is procreation, and they hold legitimate only the seeking of marital intercourse for procreative purposes. They condemn contraception and abortion.[15] In these ways, Christian rules favor the growth of population.

In addition to ideas on sexual conduct, ideas on education may affect population. All people rear their children, giving them both physical nourishment and some social training. That the rearing of children should include both intellectual and moral formation is not necessarily a given; the idea of such education is the acceptance of an option valuing the quality of offspring. The Christian theologians of the second century adopted the *paideia* of the Greeks and developed these Greek educational ideas in a new context.[16] As a result, Christians affected by this tradition were taught to value not the quantity of their children, but their quality; value was assigned not to offspring as such, but to offspring as the objects of a religious education.[17] The patristic teaching was reaffirmed in the changed culture of the Middle Ages. To seek children on a natural level, Thomas Aquinas taught, was formally sinful; only children who were to be formed spiritually constituted a good in marriage.[18] By defining the purpose of marriage as comprising both the procreation and the education of offspring, such classic Christian formulas

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produced an inherent tension. Pursuit of one goal always held the possibility of endangering the other. It was always a question of how the balance was to be preserved. The answers to this question related to the growth of population.

Another set of valuations relevant to population has centered on the rules determining freedom to marry. These rules define who can choose the spouses, what classes are eligible for marriage, when girls and boys can marry, and how marriages are made. Roman legislation presented marriage as an important social act for upper-class families, open to imitation by the lower classes, and denied to slaves.[19] The model of Christian marriage presented by the Epistle to the Ephesians was that of a free and loving union open to any Christian.[20] This model was in sharp conflict with the social norms in the early Christian centuries.

Gratian's teaching that a girl could not be compelled to marry against her will marked a distinct development in European history.[21] Very gradually, in a society where arranged matches were still the model furnished by the upper classes, the principle of freedom was applied by church courts.[22] A milestone was reached when the Council of Trent affirmed explicitly that the consent of parents was not necessary for the validity of a marriage; such marriages could still be illicit, but the great sanction of invalidity was definitively removed.[23] Yet in many European societies (such as rural Ireland following the famine of 1845), the parents' control of marriage could still be decisive.[24]

Canon law challenged the legal restrictions on slave marriages during the twelfth century.[25] Until the end of the nineteenth century, however, the practical social restrictions on slaves and then on servants inhibited freedom to marry. Economic class lines remained strong into the present century, and racial lines still dominate contemporary patterns. Yet in theory, and substantially in fact, the transition from the Roman model of marriage as a family affair for upper-class men and women to marriage as a personal choice open to everyone has been generally made in the United States and Western Europe.[26]

The Council of Trent channeled marriage toward greater control by the Church. Before Trent, marriages could be illicitly but validly contracted by the parties themselves. After Trent, only consent exchanged before the parish priest of one of the parties constituted a valid marriage.[27] This sixteenth-century increase in social-ecclesiastical control may have coincided with the beginning of an

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increase in the age at marriage. (Age fourteen for boys and twelve for girls had been established by Roman law and accepted by canon law.) The actual rise in the legal age itself, however, followed the development of a practice of late marriage. Here nineteenth-century European governments set the pattern and were followed by the Catholic Church only in 1917.[28] Nineteenth-century secular legislators and twentieth-century church legislation also swept away many of the restrictions, established by the old canon law, against marriage within certain degrees of relationship and against marriages of mixed religion. There was, paradoxically, increasing freedom to choose and increasing restriction on the age when choice might be made and on the method of expressing consent.

The demographic impact of these changing rules was doubtless mixed. A system of arranged marriages is theoretically more efficient in making sure that everyone gets married than is a system of personal choice. If, however, an important objective of the arranged marriages is to control the devolution of property, the system may function to promote only the marriages of heirs, as it did among landed groups in Europe. A rise in the age of marriage necessarily means that fewer people are physically present to marry, and that the period of marital fecundity will necessarily be reduced. Yet families marrying later may be more stable and seek to have larger numbers of children. Increase in the valuation put on the interpersonal goals of marriage decreases the value of the procreative good. Persons marrying freely may, however, have a greater interest in having children and nurturing them to maturity. The rules adopted do not, taken alone, afford a basis for estimating the net impact on population. All that is evident is the likely relevance to demographic history of the changing norms.

Of all moral concepts I suppose that the most basic is the concept of person—an individual who is entitled to be treated as I am, who has a self-determined end, who cannot be regarded as a thing. "Person" as an explicit concept has had a long and slow development. Many of the major moral controversies of the past can be analyzed in terms of who has been recognized as a person. Are aliens persons? Are prisoners, are slaves, are Negroes, are women, are infants, are embryos? Different answers to these questions have affected population history. Non-persons have often either been exterminated or restricted in their opportunities to procreate. Prisoners, for example, have almost never been treated as persons; in ancient times, this response to them resulted in their total elimina-

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tion from the population.[29] In all societies, prisoners have been restricted or prohibited from expressing their procreative desires. Infants and embryos were viewed as non-persons in the Roman world and, as such, were subject to destruction on a wide scale.[30] That women were less than persons was reflected in the long acceptance of prostitution with the concomitant of contraception practiced by the prostitutes. The willingness to have an economy where many servants were not expected to marry has existed in many societies nurturing the class belief that only people of a given status are persons. The enactment of miscegenation laws and the consequent restriction of procreative marital opportunities have been based, in part, on a belief that given races are less than fully human.

Who will count as a person is not assured nor given. Attribution of personhood may be affected by racial, religious, political, biological, economic, psychological, or social ideas. The literary lesson of *1984*—that persons may in their own lives become unpersons—is brought to contemporary life by the Chinese Communists' treatment of members of the counterrevolutionary classes. The perception of persons is a function of ideas about humanity; on this perception have hinged the generative opportunities and the existence of substantial numbers of the population.

To determine that ideas have been held on population, procreation, chastity, sterility, education, freedom to marry, and persons—all of which evidently bear in a theoretical way on demographic questions—is not to determine that these ideas have been followed or, if followed, that they have been influential. There are at least three further questions to be asked: How have the ideas been diffused? How have they been accepted? To what extent have they modified one another or been affected by other factors?

In answering the first question, it is evident that there is interaction between the actual behavior of any group and the ideas proposed to it as models by its leaders, teachers, or legislators. There may also be interaction between the pattern of those ideas so firmly established as to be unself-consciously held as folkways and that of the ideas proposed as new and better models by moral philosophers, prophets, or critics. In the history of Western thought on sexual conduct, two examples may suffice. Jesus teaches an equality between man and woman in the right to fidelity of a spouse (Matthew 19.9), when he applies "adultery" to a man's sexual conduct with an hitherto unmarried woman; a similar equality of sexual rights in marriage is enunciated by Paul in the First Epistle to the Corinthians

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(1 Cor. 7.3-5). This idea of sexual equality was counter to current behavior in the Mediterranean world. One striking example of the resistance of the popular culture to this idea is furnished by the Roman divorce legislation enacted in 331 under Christian auspices by Constantine. The legislation makes adultery by a wife ground for divorce, but explicitly provides that a husband's being a "womanizer" *(muliercularius)* is no ground.[31] The ideal of equality is thus repulsed even at the level of formal law. It is only in the twelfth century that canon law sets out the idea as at least a legal norm for the church courts,[32] and this legal proclamation is, of course, merely the beginning of the influence of the idea on behavior.

A second example is the view that there is a natural position in sexual intercourse. There is nothing in Scripture on this subject, and the Greeks and Romans do not seem to have had any strong notions about it. Beginning with the penitentials of the seventh century, however, a belief appeared that there is only one lawful position, that of the man above the woman.[33] The belief must arise from some Anglo-Saxon folkway, for no other source is apparent. The rule was then accepted by the later scholastic theologians and persisted as a norm till well into the fifteenth century.[34] The case is an instance of a model being taken from the belief or practice of one group of unsophisticated people and then generalized as an ideal.

These examples show that it would be an error to postulate a straight lineal relation of ideas proposed by moral teachers to the behavior of those taught. Yet in the area of sexual conduct, the ignorance of individuals has been very great as to what is "normal" or "right." The channels of communication have often been controlled, and high social importance has usually been attached to the basic norms of the society. In such a context, the confident setting out by leaders of certain patterns as correct has influenced behavior, however much instinct or folkway has modified their ideas. Most behavior here has been learned, however unself-consciously the learning has been carried on. Relatively small groups have concerned themselves with this conduct; they have conveyed knowledge, exemplified ideals, enacted laws, and, thus, have affected larger groups. Their impact has necessarily been in the long-run, with a time lag of short or long duration. In general, however, a large degree of rationality has not entered into the decisions of individuals when answering such questions as: Shall I get married? How many children shall I have? Shall I determine whom my child marries? These questions have been answered by individuals within

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existing structures governing sexuality, the conditions of education, and the concept of person. Through these structures, which reflect ideas modified by behavior, instinct, and earlier structure, ideas have influenced behavior. The ideas, whose dissemination is to be traced, come from both the earlier structures and the moral leaders and critics.

For the West, ideas on population from both sources have been conveyed by liturgical acts, especially the marriage service and the sacrament of penance; by didactic forms, particularly parental homily, preaching, catechism, and elementary schooling; by imaginative literature; by law, both canon and secular, acting as much as standard-setter as sanction-applier; and by personal example. By examining these multiple sources, the broad lines of diffusion of ideas may be surveyed and some estimates made as to how the ideas have been disseminated.

The second question, the extent of acceptance, is more difficult to answer. It would evidently be a mistake to measure the force of an idea by its probability to us; its power depends on its hold on the person receiving it. A bare cataloguing of relevant mythic-moral ideas such as I have attempted in this paper is apt to seem unreal; the ideas appear as illusory, unlikely, bizarre. Their persuasive power has to be measured in the situations to which they were responsive, in the cultures in which they were embedded.

Within the historical situation of the cultures, there have been different levels of acceptance. The Gospel parable of the seed and the various soils is aptly applied to the audience of any idea. There is always a range of attitudes—from impassioned conviction to sullen compliance to open defiance. A variety of investigative means is necessary to arrive at estimates of the hearers' adherence to any given concept or valuation.

In the area of religious ideas, the crudest of tools has been the label "Catholic" or "Protestant." The labels appear to be of some use for seventeenth- and eighteenth-century Europe, but, applied to contemporary societies, they are survivals of post-Reformation controversy. They have been of great interest to religious polemicists intent on attributing all the character of a country to its creed. They have been maintained in currency by ecclesiastical leaders anxious to claim as adherents 99 per cent of a country and are perpetuated by newspapers and magazines which find it paradoxical that a country can be 99 per cent Catholic and vote 30 per cent Communist.

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More meaningful efforts have been made to measure religious beliefs not by the nominal classification of an ecclesiastical census, but by liturgical practice. For Catholics, the most common criterion has been attendance at Mass; sometimes the more substantial index of frequency of confession or communion has been used. These tests are helpful in a negative way. If a person never attends Mass, it is unlikely that other specifically Catholic concepts are familiar to him. These criteria are, however, inadequate to determine if specifically Catholic concepts relating to population and procreation have been transmitted to the group investigated. Ideas are not magically imparted by a liturgical action; there must be teaching, literal or symbolic, linking the liturgical acts to particular valuations. Consequently, to measure dissemination of an idea, the means used to transmit it must be ascertained and the frequency of exposure to these means determined. Only after such determination has been made, can it be said that the label "Catholic" includes for the group investigated the clusters of concepts and valuations that formal doctrine embraces.[35]

This second question, the determination of acceptance, imperceptibly melts into the third and most difficult question, the extent to which the idea has been affected by other factors. A thousand personal circumstances bear on the way an individual receives an idea. Yet it is supposed, with some foundation where ideas of a religious character are concerned, that individuals may be grouped by social class, occupation, ethnic origin, political boundaries, historical experience, age, and geographic location. Such groupings will be relevant not only in determining the dissemination of ideas, but in estimating the probable hold of the ideas on those hearing them. Definition in terms of these seven major variables yields groups whose responsiveness to particular kinds of mythic-moral ideas may be gauged.[36]

Yet ideas are never received singly, and they are never received unchangingly. When they appear as part of a system, a complex of related concepts is imparted: The concepts balance, constrain, or modify one another. In this dynamic tension, development may be expected. It cannot be determined *a priori* which ideas will be dominant or which ideas will develop. Specific ideas on population and sexuality are only a part of a larger mass of ideas. A theology's concept of sin, for example, may be as relevant as its concept of marriage. The popular culture's approach to leisure may have as much effect on demography as its view of women. The organization—ec-

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clesiastical, political, educational—conveying the ideas may have enormous impact on both the shape of the ideas and the weight attached to them. Study of ideas does not exclude attention to the organizational, cultural, and broader theological or philosophical framework, but depends on a consideration of the interrelations among them.

To take the Christian example, who could say in advance that the model of the one-child family presented by the Holy Family would be more or less important than the Christian Fathers' Stoic doctrine on the procreative purpose of the sexual faculties? Who could suppose that of the three classic goods of marriage—offspring, fidelity, indissolubility—fidelity would be given less weight in Latin Catholic countries than procreation? Who could determine that education or procreation would be preferred in case of conflict? To say that one value or the other should have prevailed is to theologize. In writing history, one can only point to the existence of balancing ideas in a system and then investigate which ones became dominant in the local experience of the particular group examined.

If a group is defined by its multiple nonideological characteristics, if the dissemination of ideas to that group is determined, and if the dominant ideas are observed with allowance both for the counteracting force of other ideas in the system and for their own development, it seems possible to relate the effect of the ideas received to the population history of the group. Such a relation will only be a matter of reasonable hypothesis. To use these reasonable hypotheses, to take them into account as factors in population history, seems to me preferable to writing a history that, by ignoring all but the economic factors, tacitly supposes that economic rationality alone is real.

III

Three examples may serve to illustrate how demographic history might be enriched by more attention to intellectual history. First, consider the increase of abortion in Hungary in the 1950's. Therapeutic abortion boards were established in Hungary in 1952. On June 3, 1956, the principle of abortion on demand was legally established. Describing these two legal events, Christopher Tietze asks: "What has been the effect of the legalization of abortion in Hungary?"[37] He then provides the following figures:

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Abortions per 1,000 of Population

	Legal Abortions	Other Abortions
1949	.2	3.4
1952	.2	4.4
1955	3.6	4.4
1956	8.3	4.2
1957	12.5	4.0

The only factors correlated by Tietze with this striking increase in abortion are the changes in the law. These legal changes are surely relevant, but laws are signs of ideas as well as causes in themselves. The spread of Communism among Hungarian workers, the suppression of the religious orders and most religious instruction, the increasing desperation of elements of the population in the mid-fifties, the ideas that burst into action in the October, 1956, revolution, the ideas that were generated by the failure of the revolution—all these intellectual events are also relevant. The two legal events afford a misleading account of the changes in the behavior pattern unless they are placed in the context of this political and intellectual ferment.

Secondly, consider a larger example: the demographic history of Ireland before the famine. In *The Population of Ireland, 1750-1845,* K. H. Connell estimates the Irish population at something over four million in 1781, postulates its doubling (despite substantial emigration) by the time of the great famine, and hypothesizes that the population increase was due to substantially earlier marriages.[38] He then gives reasons for earlier marriages in largely economic terms. The cultivation of the bog, the swing from pasture to arable land, the increase of agricultural exports, the general dependence of the natives on the potato, and the subdivision of holdings made early settlements of estates possible and therefore promoted marriage for men at age twenty or twenty-one[39]; the early marriages resulted in the population increase. He adds that "children were wanted because of their unusual value to their parents" as cultivators of the land.[40] The population increase was higher in counties like Connaught than in counties like Leinster: Connell makes the supposition that a near-subsistence economy in Connaught encouraged the improvidence of early marriage more than the greater prosperity of Leinster.[41] His general hypothesis is that greater prosperity stimulated marriages and births, but that when a certain level

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of prosperity was reached, other economic calculations led to some postponement of marriage and a consequent slight decline in the birth rate.

That many people married as early as age twenty in pre-famine Ireland is doubted by Michael Drake. The Malthusian beliefs of literate observers, he suggests, often lead them to see early marriages as the cause of poverty, but their casual observations are not decisive. Nor can travelers' exclamations at low ages in exceptional cases be taken as evidence of a trend.[42] The Census of 1841 shows that the median age of bridegrooms in the 1830's must have been twenty-five, and there is no substantial evidence showing that it was earlier in the two generations before 1830.[43] Drake suggests instead that the population increase was due to the spread of a potato diet between 1740 and 1780, which resulted in a lowering of mortality and a substantial increase in fecundity.[44]

This controversy between Connell and Drake has been properly conducted in terms of the meaning of the census and observer evidence. When causes for change or stability are to be postulated, however, there also seem to be political and intellectual events relevant to a history which gives reasons as well as numbers. The period described by Connell is the beginning of the resurgence of the Irish nation after a century of colonial domination. Its beginning is marked legally by such enactments as "the Bogland Act" of 1771 permitting some land-leasing by Catholics; the recognition of legal rights thus begun culminates in the Catholic Emancipation Act of 1829. The laws are both the effects of changing political ideas and the causes of new changes. The abortive revolt of 1798 is both a sign of desperation and a cause for despair of successful violence. Connell relates the effects of the Grattan parliament and the Napoleonic wars solely to the increased economic activity. He describes Catholic emancipation only in terms of its effect in making possible a Catholic middle class. He refrains from any suppositions about the effect the political changes had on thought affecting marriages and births. That the ideas which necessitated political change had repercussions on attitudes toward population does not, however, seem improbable. To consider only the economic effect of such major political and ideological changes in a country stirring with the consciousness of its oppression seems to shut out evidence of potential significance to the history of the birth rate.

The relation of religious ideas to the population increase also seems worthy of consideration. In the mid-eighteenth century, the

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outlawed bishops lived the lives of itinerants staying in "places of refuge"[45]; the parishes were irregularly supplied; the religious orders were criminal; Catholic education consisted in the uncertain efforts of the "hedge schools."[46] Clerical candidates (some 480 in all)[47] were educated abroad without reference to the language or history of their country and could not have provided more than seventy new priests a year.[48] Priests had little secular influence on their flocks.[49] A change was already under way by 1782 when the Catholic Relief Act was enacted, permitting Catholic churches to be built within towns, Catholic bishops and members of religious orders to live in Ireland, and Catholics to teach school if licensed by a Church of Ireland minister. Seventy years later, by 1850, the bishops were a powerful body firmly based on domestic soil; the national seminary, St. Patrick's College at Maynooth, had supplied 1,272 of the 2,291 Irish clergy; the parishes were well-staffed, the religious orders thriving; Catholic churches were openly and legally attended; an effort at Catholic elementary education had begun.[50] The Maynooth priest had become the dominant figure in the villages and small towns. He was now, a friendly writer declares, "the teacher, the guide, and the counsellor of his people, their leader in social and political movements, the center of their energies, the focus of their power, the exponent of their ways, the moderator of their excesses."[51] Did such powerful local leaders have no effect on attitudes toward marriage and toward birth?

The formal ideas of the Irish clergy on sexual morality underwent no change in this period. In the eighteenth century, 75 per cent of them had been educated in France and another 15 per cent in the Low Countries.[52] Rigorism in morals dominated the seminaries of these countries. When Maynooth was founded, the same traditions were continued. The first professor of moral theology was a Frenchman, Louis De la Hogue (1798-1801), followed by an Irish graduate of Paris, Edward Ferris (1801-1809), followed by another Frenchman, Francis Anglade (1810-1828). These men devoutly maintained Augustinian sexual teachings as the law of God.[53] The textbooks in use, Antoine and Bailly, were among the most severe of moral treatises. The theory expounded by these books and by these professors condemned any premarital sexual activity, restricted sexual initiatives in marriage to the procreative act, and viewed the whole sexual area of human life as peculiarly prone to concupiscence and actual sin.

Some of the ideas thus conveyed favored marriage and procrea-

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tion. The restriction of lawful sexual activity to marriage encouraged marriage if it was economically feasible. The view that marriage was meant for procreation encouraged copulation for population in marriage.[54] Contraception and abortion were shunned. Values counter to early marriage and procreation were, however, also being transmitted. The attitude on sexual activity before marriage discouraged courtship and the occasions for the social meetings of boys and girls. Sexual incontinence was sternly rebuked.[55] A sterile wife was a disaster, but the expedient of some peasant societies of establishing a woman's fertility in premarital intercourse was not adopted.[56] The narrow limits placed on sexual activity in marriage, the insistence on the indissolubility of a marriage once made, and the high premium put on chaste celibacy all worked against any acceptance of early marriage as an eminently desirable state. Copulation control was itself a form of population control in diminishing the number of chances in which conception could occur. The valuation the clergy placed on the education of children, reflected in the strenuous efforts to establish Catholic schools, embodied an approach to childbearing that stressed quality as well as quantity of offspring.[57] In all these ideas discouraging early marriage and early procreation, Ireland is but a more pronounced case of the Western European marriage pattern, which I shall set out as my third example.

Can one conclude from the existence of ideas working both for and against early marriage that they canceled one another out? Such an approach would seem unduly mechanical, insufficiently sensitive to the dominant trends of the period. Connell, however, only remarks that the people were reputed to be continent, that they believed in marriage, and that they did not practice contraception. Otherwise he attempts no correlation of moral ideas and demographic history. His later account of peasant marriage after the famine focuses on the economic motives for marriage; and in the one place where he mentions moral attitudes, clerical discouragement of courting is seen as a reinforcement of the economic desire to postpone marriage.[58] His view of the peasant would suggest that the economic aspects of life so effectively controlled decisions on marriage that the moral framework was irrelevant. Alternatively, he may have assumed that, as the moral ideas were unchanged throughout the period studied, they might safely be ignored as a factor causing change.

There was, indeed, in the period 1750-1845, no change in the

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formal ideas imparted to the Irish clergy and presumably no change in the ideas of sexual morality actually conveyed by them to the people. There was, however, a change in the role, training, numbers, and importance of the priests, a change noticeable some time before 1782 and marked after the foundation of Maynooth in 1798. This change in the status of the conveyors of teaching on sexual behavior is a factor that cannot be treated as nonexistent.

In this short compass, the impact of the ideas conveyed by the clergy can only be raised as a possibility. Parish by parish information would be necessary to trace the effect definitively. Nevertheless, it seems relevant to population history to inquire if this clerical influence helped to establish a pattern of increasingly later marriages well before the famine. The lower birth rate of Leinster, for example, might be associated not only with economic prosperity, but with more extensive efforts on the part of clerics to educate the people. An account of Irish population changes that does not consider the net effect of ideas favoring and discouraging marital procreation seems to have the burden of showing the irrelevance of the ideological elements in an era when this complex of ideas was being conveyed with increasing vigor, by an increasingly strong ministry, to people responsive to ecclesiastical leadership.

My third example is the relation of marriage in Western Europe to the Christian ethic. Hajnal pointed to the existence of a marriage pattern in Western Europe distinct from that in Eastern Europe, Asia, or Africa.[59] The picture drawn by him is now being confirmed by the detailed studies of Ansley Coale and his associates.[60] The pattern is that of a society distinguished by two features: Marriages for both men and women occur at a relatively late average age; many persons do not marry. In these two aspects, Western Europe is distinguished from societies where almost everyone gets married and almost everyone gets married by age nineteen or twenty. This distinctive pattern begins to develop slowly in Europe, probably by at least the seventeenth century; it is strongest in nineteenth-century Europe, where Coale's work, based on census reports, is able to show fluctuations on a province by province basis. It is common to both "Catholic" and "Protestant" countries, but not to Eastern Orthodox countries. It obtains more or less regardless of an area's degree of industrialization. It is specific enough to Western Europe and general enough within Western Europe to ask for an explanation that will fit Western Europe as a unit. A hypothesis, then, is needed that is not related to sectarian religious differences, nor to exclu-

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sively economic factors, nor to specifically national characteristics. Such a hypothesis is one relating the pattern to the cluster of ideas constituting Western Christian sexual morality.

These ideas are sixfold: the confinement of sexual intercourse to marriage, the treatment of marriage as monogamous, the emphasis on the ideal marriage as permanent, the justification of marital intercourse by procreative purpose, the condemnation of contraception and abortion, the insistence on the education of children. These central Christian ideas were present, with slightly different emphases, in both Catholic and Protestant cultures.[61] They combined to encourage both sexual restraint and postponement of the serious step of entering marriage and raising a family. They operated against the background of a religion whose founder was presented as celibate. They produced a very different approach to marriage than that of, say, the Talmudic teaching to marry young in order to avoid impure desires or the secular injunction to marry young to ensure heirs to one's property. They did not, of course, operate alone; they were reinforced by other ideological, political, economic, and physical influences; nevertheless, they were the common denominator upon which other influences produced variations without altering the basic pattern.

Negative confirmation of this hypothesis is found in the changes which had already begun to occur at the end of the nineteenth century. By then, for example, in the most anticlerical portions of France, the marriage age had fallen significantly as had the fertility per marriage.[62] In these areas the relevant ideas on desirable morality had been rejected or partially rejected by the populace. The Western European pattern changed more generally in the next sixty years. This development corresponded both to a rejection of Christian ideas by much of Western mankind and to changes, such as the acceptance of contraception by the Protestant churches, in the formal Christian ideas themselves.

Why did the pattern occur in post-Reformation rather than in medieval Europe? The timing reflects the time lag between the acceptance of ideas by the moral teachers of Europe and their diffusion by law, literature, liturgy, preaching, and example. Only in post-Reformation Europe did the education of a parish clergy in both Catholic and Protestant countries assure the dissemination of the formal ideas.[63] To take two other instances of this kind of time lag, it has been argued that the witchcraft hysteria of the late-fifteenth and early-sixteenth centuries reflected the effect of the pre-

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ceding several centuries of theological work in elaborating a theory of witchcraft and providing detailed information on the behavior of witches.[64] The virulent outbreak of anti-Semitism in twentieth-century Germany has been related to the conditioning of attitudes by centuries of Christian theologizing in which the Jews played the roles of scapegoats or villains.[65] In both these cases, the theological notions involved were relatively peripheral to the main body of doctrine—indeed, were contradictory to more central Christian ideas. Yet it is plausibly contended that witchcraft theory and anti-Semitic theology, detached from the central Christian concepts, powerfully affected popular behavior centuries after the ideas were put forward by the theological elite. If peripheral elements of Christian theology could work so strikingly for ill in these popular manifestations, it is not unreasonable to suppose that more central ideas operated with profound, if delayed, effect on European behavior to influence sexual conduct along the main lines to which the Christian moralists had so seriously committed themselves.

Why was there such a marked difference between Western and Eastern Europe? One factor to explain this difference might be found in the greater vitality of the parish ministry in the West and the greater interest of the Western clergy in affecting moral conduct. The institutions of preaching in the Protestant countries and of confession in the Catholic countries were not matched in the East. A tendency to restrict asceticism to the monasteries and a relatively more tranquil acceptance of human failings in the secular Christian also marked the Orthodox of the East. In addition, two doctrines were treated differently in the East: Divorce was accepted, and the procreative purpose of intercourse was not stressed. In sum, both the complex of ideas on sex and marriage and their communication were significantly different in the hands of the Orthodox. The differences, when combined with many nonideological factors, help to explain the different pattern.

The general hypothesis of any relationship between the formal Christian ideas and the Western European pattern and my answers to specific difficulties with this hypothesis are put forward only tentatively. I hope that they are plausible enough at first sight to provoke refutation or confirmation. They may serve, at least, to suggest the relevance of intellectual history to a specific demographic pattern.

It has been the discovery of the historians of legal institutions

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that such history cannot be written without more attention to general cultural and political factors than they were at first prepared to give. Demographic history would seem likely to undergo a similar expansion of its concerns. What it has to deal with is evidence of human behavior. That behavior points to thoughts. To account for that evidence, to integrate it, to understand it require a history not only of births and deaths, but of ideas, if the history is to be a history of men.

References

1. *Niddah* 13B, *The Babylonian Talmud,* ed. Isidore Epstein (London, 1935-1952).
2. See Eusebius, *The Gospel Demonstration* 1.9, *Patrologia graeca* (hereafter *PG*) 22:77-81.
3. St. Augustine, *De civitate Dei* 22.1, *Corpus scriptorum ecclesiasticorum latinorum* (hereafter *CSEL*) 40:583. Again in Duns Scotus, *In libros sententiarum,* Paris Report, 4.28.
4. St. Ambrose, *Expositio in evangelium Lucae,* 1.30, *CSEL,* 32^4:29.
5. For a twentieth-century expression of this belief, see John A. Ryan, "The Moral Aspects of Periodical Continence," *Ecclesiastical Review,* Vol. 89 (1933), p. 34.
6. See St. Jerome, *Contra Helvidium* 21, *Patrologia latina* (hereafter *PL*), 23:215.
7. See St. Augustine, *Contra epistolam quam vocant fundamenti* 5, 13, 15, *CSEL* 25^1:197, 209, 212.
8. See St. John Chrysostom, *On Those Words of the Apostle "On Account of Fornication,"* 3, *PG* 51:213.
9. First Epistle to the Corinthians 7:7.
10. E.g., St. Ambrose, *De virginibus,* 1.23, 38, 52, *PL,* 16:195, 199, 203.
11. Matthew 5:27-28; Romans 1:24-27; First Epistle to the Corinthians 6:16; First Epistle to the Thessalonians 4:4.
12. E.g., Canon 33 of the Council of Elvira in 306 forbids priests to have intercourse with their wives: *Sacrorum conciliorum nova et amplissima collectio,* ed. G. D. Mansi (hereafter Mansi), 2.11.
13. See Lucien Legrand, *The Biblical Doctrine of Virginity,* p. 29.
14. See St. Augustine, *De bono coniugali* 7, 29, CSEL 41:196, 227.
15. On procreative purpose, see Clement of Alexandria, *Paedagogus* 2.10.95.3,

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Die griechischen christlichen Schriftsteller der ersten drei Jarhundert (hereafter GCS) 12:214. On abortion, see the *Didache*, 5.2.

16. See Werner Jaeger, *Early Christianity and Greek Paideia* (1961), pp. 37-67.

17. Clement of Alexandria, *Stromata* 3:15, *GCS* 15:241; St. Augustine, *De bono coniugali* 8.8, CSEL 41:198.

18. St. Thomas Aquinas, *In libros sententiarum Petri Lombardi* 4.31.2.2, reply to objection.

19. *Codex Justiniani* 5.5.3, 9.11.1.

20. Epistle to the Ephesians 5:25-33.

21. Gratian, *Decretum*, Part 2, C.32, Q.2, c.1, in *Corpus juris canonici*, ed. Friedberg (1879-1881).

22. See, e.g., *Decretales* 4.1.15, 21, 29, *Corpus juris canonici*.

23. Council of Trent, Session 24, November 11, 1563, "Canones super reformatione circa matrimonium," c.l, "Tametsi," *Concilium Tridentinum* (ed. Societas Goerresiana, 1955), Vol. 9, p. 968.

24. K. H. Connell, "Peasant Marriage in Ireland: Its Structure and Development Since the Famine," *Economic History Review*, 2d Series, Vol. 14 (1961-1962), p. 513.

25. Gratian, *Decretum*, Part 2, C.29, Q.2, c.l.

26. As is suggested by studies, such as Alain Girard's, the actual range of choice, at least as those now married recalled the range they had, may still be small. The family will play a role by implicitly approving the decision made. See Girard, *Le choix du conjoint* (1964), pp. 128, 156.

27. *Loc. cit.*, note 23 *supra*.

28. *Codex canonici juris*, canon 1067.

29. E.g., Joshua, 11:8.

30. See Tertullian, *Ad nationes*, 1.15, *CSEL* 20:85; Seneca, *De ira*, 1.15.

31. *Codex Theodosianus*, ed. T. Mommsen (Berlin, 1885), 3.16.1.

32. Gratian, *Decretum*, Part 2, C.32, Q.4, c.1-10.

33. St. Bede, *Penitential*, 3.38; Egbert, *Penitential* 7.16, *Die Bussordnungen der abendlandischen Kirche*, ed. F. W. H. Wasserschleben (Hulle, 1851).

34. E.g., St. Bernardine, *De religione christiana*, 17.1.1 in *Opera omnia* (Quarrachi, 1950), Vol. 1.

35. For example, a recent study of abortion in Chile makes an effort to correlate Mass-going, confession-going, reception of the Eucharist, and the practice of abortion, but it makes no examination of the dissemina-

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tion of the church's teaching on abortion. See Mariano Requem, "Social and Economic Correlation of Induced Abortion in Santiago, Chile," *Demography,* Vol. 2. (1965), p. 41.

36. See Gabriel Le Bras, *Etudes de sociologie religeuse,* Vol. 2 (1956) pp. 547-653. Substantially the same point is made by Louis Chevalier, "Towards a History of Population," *Population in History,* eds. D. V. Glass and D. E. C. Eversley, p. 75, reprinted from *Population* (1946).

37. Christopher Tietze, "The Demographic Significance of Legal Abortion in Eastern Europe," *Demography,* Vol. 1 (1964), p. 123.

38. K. H. Connell, *The Population of Ireland, 1750-1845,* p. 51.

39. *Ibid.,* p. 89. Connell speaks of "early" marriage as around age twenty or twenty-one for men in "Marriage in Ireland After the Famine: The Diffusion of the Match," *Journal of the Statistical and Social Inquiry Society of Ireland,* Vol. 19 (1955-1956), pp. 82-83.

40. *The Population of Ireland, 1750-1845,* p. 246.

41. *Ibid.,* p. 249.

42. Michael Drake, "Marriage and Population Growth in Ireland, 1740-1845," *Economic History Review,* 2nd Series, Vol. 16 (1963-1964), pp. 303-305.

43. *Ibid.,* pp. 303, 311.

44. *Ibid.,* pp. 311-312.

45. See, e.g., the letters of eighteenth-century bishops of Meath in A. Cogan, *The Diocese of Meath,* Vol. 3 (1867), p. 166.

46. For contemporary comment on the efforts at educational reform in the nineteenth century through the suppression of the hedge schools by Catholic bishops, see the letters of James Doyle, Bishop of Kildare to the Earl of Derby, Chief Secretary for Ireland, January 13, 1831, in W. J. Fitzpatrick, *The Life, Times and Correspondence of the Rt. Rev. Dr. Doyle,* Vol. 2, p. 232.

47. See Letter of the Secretary of the Trustees of St. Patrick's College, Maynooth to the Under-Secretary in the Civil Department of the Chief Secretary's Office, Dublin Castle, in John Healey, *Maynooth College; Its Centenary History* (1896), p. 616.

48. The calculation is a rough one. It supposes a distribution over five years of study, and a certain number of ordained priests remaining abroad in chaplaincies.

49. The observation of Edmund Burke, a man perhaps interested in discounting Catholic clerical influences in Ireland, but a generally fair and intelligent observer. See Edmund Burke, *Correspondence,* Vol. 3, p. 34.

50. Healey, *Maynooth College . . . ,* p. 466.

51. *Ibid.,* p. 238.

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52. See note 47 *supra*.

53. On rigorism at Maynooth, see Healey, *Maynooth College . . .*, pp. 283-84.

54. As James Joyce sees the message conveyed: "Copulation without population! No, say I!" (*Ulysses* [1961 ed.], p. 423).

55. See, for example, the record of episcopal visitations between 1780 and 1801 of Bishop Plunkett of Meath, in A. Cogan, *The Diocese of Meath*, Vol. 3, pp. 300-340. Typically Bishop Plunkett notes in Dunshauglin on August 16, 1801, "scandalous incontinence inveighed against and threatened with the divine vengeance."

56. Connell, "Peasant Marriage in Ireland: Its Structure and Development Since the Famine," p. 517. Connell's observations made here on post-famine Ireland seem equally true of pre-famine Ireland.

57. In the same visitation record of Dunshauglin, referred to in note 55, Bishop Plunkett also denounced "neglect of the Christian education of children." On the bishops' effort for Catholic schools, see Fitzgerald, *The Life, Times and Correspondence of the Rt. Rev. Dr. Doyle*, Vol. 2, p. 232.

58. Connell, "Peasant Marriage in Ireland," p. 572.

59. J. Hajnal, "European Marriage Patterns in Perspective," *Population in History*, p. 106.

60. See Ansley Coale, "The Decline of Fertility in Europe from the French Revolution to World War II." Paper presented at the Conference on Historical Population Studies, Bellagio, Italy (July 7-13, 1967). The paper will be published with the title "Fertility and Family: A World View," as part of the Sesquicentennial Celebration of the University of Michigan (forthcoming).

61. "Catholic" and "Protestant" are used here to denote cultures which have self-consciously different ecclesiastical organizations. The difference in other respects needs close analysis. E.g., in "Protestant" England divorce with right to remarry was not made generally available before the nineteenth century. In some sociological writing there is over-emphasis on Weber's concepts of "the Protestant ethic," despite his own later severe qualifications of the idea and his own later approach to the special characteristics of European capitalism in more general terms of a spirit of rationality. See Max Weber, *Wirtschaftethik der Weltreligionen in Gesemmelte Aufsatze zur Religions Soziologie* (1920), pp. 536–46.

62. Ansley, Coale, "The Decline of Fertility in Europe. . . ."

63. On the very slow development in the seventeenth century of a seminary-educated clergy, see Paul Broutin, *La Réforme pastorale en France au 17e siècle* (1955), pp. 11, 89, 296.

64. Hugh Trevor-Roper, "Witchcraft and Witches" *Encounter*, Vol. 28 (May, 1967), pp. 3-25.

65. Georges Friedmann, *The End of the Jewish People?* (English translation, 1967), p. 268.

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ETIENNE VAN DE WALLE

Marriage and Marital Fertility

THE RELATIONSHIP of marriage to general fertility is at the heart of the preoccupations of nineteenth-century demographers in Western Europe. During his stay in Switzerland, T. R. Malthus met a peasant who "appeared to understand the principle of population almost as well as any man I ever met. . . . [The] habit of early marriages might really, he said, be called *le vice du pays;* and he was so strongly impressed with the necessary and unavoidable wretchedness that must result from it that he thought a law ought to be made restricting men from entering into the marriage state before they were forty years of age, and then allowing it only with *des vieilles filles,* who might bear them two or three children instead of six or eight." Malthus relates, however, that he "found upon inquiry that [the man] had himself married very young."[1]

Marriage and the Birth Rate

Although no law so extreme was ever passed, there were many restrictions to marriage at that time in Swiss cantons, German states, and elsewhere. Some of these laws had been passed long before Malthus, when there was a general belief in the importance of population growth. Although it was acknowledged that a large population was necessary for the welfare of the state, marriage laws were instituted to protect local finances against the prolificacy of irresponsible paupers who might otherwise have a claim on relief or charity. During the first half of the nineteenth century, under the influence of Malthusian ideas, marriage restrictions were reactivated.[2] They were intended to relieve the pressure of population by compelling the poor to "moral restraint." In Switzerland, restrictions of marriage on economic grounds were abrogated only

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with the passage of the federal constitution of 1874 and the civil status law of 1876. Until then, various regulations had been effective in barring a large proportion of the population from marriage. For instance, in the canton of Lucerne, where the restrictions on marriage were particularly pervasive,[3] the proportion of women single at age fifty reached 39 per cent in the Census of 1870. After age fifty, the chances to marry are slight and, in any case, fertility is not affected. A population in which almost 40 per cent of the women of childbearing age are unmarried will find its birth rate seriously lowered, numerous illegitimate births notwithstanding.

Legal restrictions on marriage, whether introduced before or after Malthus, created a general atmosphere of restraint, which had to be borne by a large proportion of the population. The older generation was obviously worried by the early marriage of the young. Malthus's hostess in a Jura inn complained "that boys and girls were marrying who ought still to be at school."[4] The conflict between the generations may have been the source of the belief, in the absence of adequate statistics, that the age of marriage was, in fact, going down.

In an earlier era of populationists, J.-L. Muret, the minister of Vevey in the canton of Vaud, worried about a decrease in the number of people; he inferred this from a study of parish registers and attributed it mostly to out-migration. In 1764, he tabulated data from the marriage registers of his parish which showed the age of marriage among women to be 26 years and 2 months.[5] He wrote at the time:

> A means which would seem very efficient, and be the more appropriate since it would enhance population more directly, would be to bar from all employments the non-married men; to tax inheritance accruing to a single man aged 25 years and over; and to favor in various ways those having the most numerous families.[6]

Half a century later, Dean Bridel, the minister of Montreux in the same canton, spoke favorably of the region's low birth rate. He computed the age at first marriage of women in his parish at 26 years and 9 months, a figure very close to that indicated by Muret's data, and he commended late marriage as the cause of the low birth rate. He wrote:

> The obvious, overriding and, to my eyes, sufficient cause to explain it is a pronounced, general and already ancient tendency towards postponing as much as possible the age of marriage. . . . Where women rarely become mothers before 28, 29 or 30 years, they have acquired the ma-

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turity needed to give their infants more intelligent and conservative care than very young mothers. In fact, they also have a lesser number of children, since, at least in my parish, they consider it a duty to nurse them for a long time; this leaves less margin for pregnancies, and results in the latter following on one another at intervals of three years.[7]

Whether writers deplored too early marriage or the excessive postponement of marriage—whether they rejoiced because women married early, thereby checking depopulation, or married late, thereby indicating forethought and restraint—they all agreed that the "incidence" of marriage was the main factor affecting fertility. By the incidence of marriage, we mean the combined result of the age at marriage and the proportion who never married. In many European countries, the two combined to limit to 35 to 40 per cent the proportion of women of fecund age that were married. This pattern of marriage, which probably had no equivalent elsewhere in the world and has now disappeared everywhere except Ireland, markedly lowered the birth rate in Western Europe.

The possibility of fertility control within marriage was hardly ever mentioned, even in France, before the second half of the nineteenth century. Such writers as Cauderlier attempted to refute evidence which might have suggested the contrary. As late as 1900, Cauderlier denied the importance of "artificial causes of fertility limitation."

I think these causes have always existed, and I don't see very well why they would suddenly have become more powerful, to the point where they would have caused the recorded decline in the French birth rate. . . . In brief, the fertility of women is a constant in time; in other words women are as fertile today as fifty years ago. The increase and the decrease of marriages are sufficient to account for the greatest demographic phenomenon of the last half-century.[8]

There is ample evidence in nineteenth-century Europe to indicate a close association between fluctuations in the number of births and fluctuations in the number of marriages. Economic conditions—depression and prosperity, drought and good harvests—affected the number of births by influencing the number of marriages. Many authors of the time attempted to relate nuptiality and fertility to the price of staples, particularly grain. The relationship of marital fertility (as opposed to the birth rate, the number of births over the total number of persons in the population, be they married or not) to the incidence of marriage deserves to be examined further.

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Strategies of Family Formation

Control of marital fertility by contraception, as we know it today in Western countries, is without doubt a fairly recent development. It probably did not appear on a large scale much before the Revolution in France, and 1870 or later in most European countries. Moreover, in most of the world today and even in Europe in the past, uncontrolled fertility has most likely been accompanied by a pattern of early and universal marriage, at least among women.

According to J. Matras's scheme, there are four conceivable "strategies of family formation," combining either early or late marriage with either uncontrolled or controlled fertility.[9]

	Uncontrolled Fertility	*Controlled Fertility*
Early Marriage	Strategy A	Strategy B
Late Marriage	Strategy C	Strategy D

Strategy A is still the rule in most of the world, as it was in Europe during Antiquity and the Middle Ages. Strategy C was advocated by Malthus and adopted, on the whole, in Western Europe in the nineteenth century. J. Hajnal argues convincingly that there had been a shift at an earlier date from Strategy A to Strategy C; from a pattern of early and general marriage to what he calls the European (meaning Western European) pattern, characterized by high age at marriage and a large proportion of people who never marry when fertility is uncontrolled.[10] One might speculate that the adoption of Strategy C was a preliminary step in the development of fertility control within marriage. Eastern Europe, however, underwent a fertility decline without abandoning the custom of early and almost universal marriage.

The transition from Strategy A to C occurred at an unknown date, and there are not many statistics to document it. We are concerned here only with the transition from Strategy C to Strategy B. In several countries, statistical records (censuses and vital registration) are available for the period during which this shift took place. We shall examine the evidence for four countries in which the transition has apparently occurred—namely, France, Switzerland, Belgium, and the Netherlands.

The two paths from C to B on Matras's diagram (the first is by way of A, the second by way of D) correspond to two theories on the relationship between changes in fertility and those in marital

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habits. The first theory follows the lead given by economic historians. H. J. Habakkuk and W. L. Langer, for example, contend that population growth in eighteenth-century England may have been due not to mortality decline, but to a decline in the age at marriage and to the resulting increase in the number of births.[11] Since this theory relates to a time when vital registration and censuses provide no usable information on the topic, we cannot test it here. We can only say that the proportion of single women in the beginning of the nineteenth century was extremely high in those areas of Europe which we are considering (with the exception of large parts of France) and that it is unlikely that it could ever have been much greater.

The Dutch historian E. W. Hofstee, however, has invoked a similar argument to explain the recorded increase in the birth rate of the Netherlands between 1850 and 1875, a period covered by reliable national statistics. Hofstee attributes the rise to an increase in the frequency of marriages as the population departed from what he calls the "classical agrarian-artisanal pattern of reproduction."[12] According to this view, marriage had previously depended on the possession of a house and the means of livelihood. Among rural populations, the eldest son would often be the only one who could afford to marry, and then only when his father had died or was too old to take care of the farm. Younger brothers and unmarried sisters would live on the farm as dependents with ancillary status. During the "proletarian transition period," the old institutions started breaking down as a result of industrialization and urbanization. The ties became looser between employers and servants—or unlanded members of the family, the two often being synonymous. People married earlier and in larger proportions; they went to towns where they were able to earn a living in ways other than agriculture. The number of holdings ceased, therefore, to limit the number of households to the extent that it had during the earlier agrarian period. The birth rate grew, and many of the old values based on tradition were abandoned. A dynamic pattern of culture fostered the acceptance of fertility control, and the birth rate began a sustained decline.

Hofstee's theory is of interest here because it describes a sequence of events where changes in marital customs antedate a decline in marital fertility. (The proportions married increased during the nineteenth century in all parts of the area we are considering.) To the extent that the theory attempts to make this shift

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a decisive feature in a general succession of historical changes, it is vulnerable to many objections. Was there ever an agrarian-artisanal pattern of reproduction in Europe? If so, when did it start to disappear? The European pattern of marriage appeared relatively late, if Hajnal's interpretation of the historical evidence is correct. The Dutch population in the Middle Ages, for example, probably married early and universally. On the other hand, when did "proletarization," as Hofstee calls it, occur? There must have been previous changes in the proportion married. Such changes in nuptiality happened to coincide with the beginnings of national population statistics in the Netherlands, but it is questionable whether the recorded changes can be attributed to fundamental and irreversible social trends occurring decisively in a relatively well-defined period.

The second theory put forth to explain the transition from Strategy C to Strategy B entails passage through an intermediary stage (Strategy D) in which fertility is controlled and marriage is late. This hypothesis holds that recent declines in age at marriage were made possible by the substitution of contraception for late marriage as the means of curbing population growth. Joseph Spengler, for instance, has employed this hypothesis in the case of France:

> Evidently men have tended to marry in relatively greater numbers as they have acquired the practice of family limitation. Or, stated in other words, in France the population has always (since 1860 at least) sought to limit natural increase, either by deferring and/or avoiding marriage, or by limiting fertility within the married state.[13]

It has sometimes been argued that the European pattern of marriage disappeared because late marriage had lost its function, which was to keep the family small.[14]

The Course of Nuptiality and Marital Fertility

We can gain some insight into the validity of the theories described by comparing the trend of the age at first marriage and of the proportion never married among women in France, Belgium, Switzerland, and the Netherlands during the period covered by national statistics with the trend of marital fertility. (See Figure 1.) It is sufficient to say that age at first marriage is estimated from the proportion of single women at each age in the censuses, comparing one census to the next; that the proportion of women

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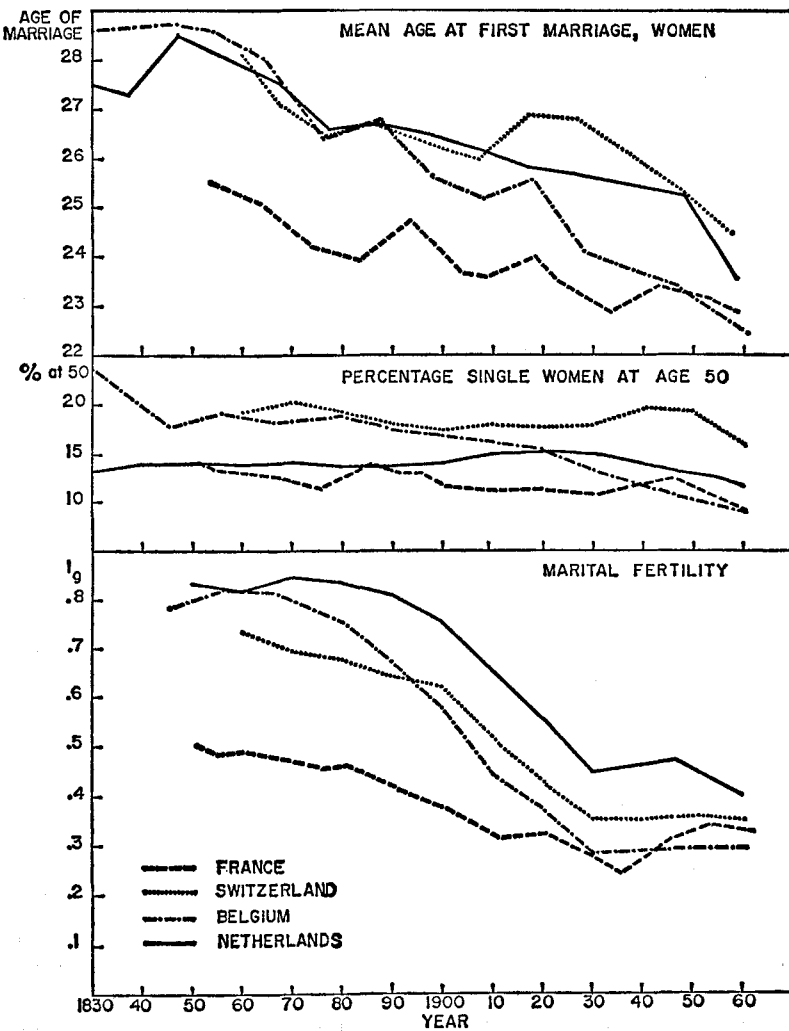
never married around age fifty is a good measure of the proportion who never marry; that our index of marital fertility is free of the influence of age differentials in the populations considered, and that it indicates marital fertility as a proportion of the highest fertility ever reliably recorded in a human population (which would be equal to 1).[15]

Figure 1



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The decline of marital fertility in Switzerland, Belgium, and the Netherlands after 1850 is almost surely the result of large-scale adoption of family limitation; the decline began much earlier in France. The statistical record encompasses the transition from high to low marital fertility (including some of the pre-decline plateau) in Belgium and the Netherlands, but not in France and Switzerland.

In all instances, the mean age at first marriage shows a marked tendency to decline. By computing from censuses ten years apart, extreme fluctuations are evened out. There is a suggestion of a rise in Holland and Belgium at midcentury caused by the economic crisis of 1846-48. A potato blight struck at this time and represented a major catastrophe not only in Ireland, but also on the Continent. All four countries show an increase in age at marriage during the 1880's. This led some writers of the time, Cauderlier among them, to speculate that fertility decline was caused by delayed marriages. In retrospect, the change in age at marriage clearly appears to have been only a fluctuation in a secular trend which resulted most probably from economic causes. The trend is interrupted by an occasional economic crisis and by World War I. Fertility, however, continued to decline despite the ups and downs in age at marriage. Cauderlier believed that marital fertility was a constant and failed to realize that it had started to come down.

Age at first marriage declined almost steadily in the four countries for more than a century. France was only exceptional in that its population married at a much younger age than did those of the other countries considered here. Age at marriage might possibly have come down before the first French census for which we possess an age and marital status distribution, that of 1851. Knowledgeable students of French population history, however, argue against this proposition. Louis Henry has compared the proportions single given by Moheau in 1778 for certain regions of France at the end of the *Ancien Régime* with those given for the whole country in the 1851 Census; if anything, the proportions single were higher in 1851.[16] The study of parish registers indicates an age at first marriage of about twenty-five.[17] Of course, the effect of the Revolution and the Napoleonic wars on nuptiality cannot be estimated. The proportion married might have become smaller after this period of hardship than it had been during the *Ancien Régime*. War losses, by diminishing the supply of marriageable males, tend to decrease the proportion of married women.

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Finally, the percentage of women never married appears to have remained stable during the period under review. Belgium alone shows a continuous and slow decline. Fluctuations from census to census are minor, and the last census indicates an all-time recorded low everywhere. Although the decline in age at first marriage is notable in practically every administrative subdivision (*département*, canton, or province) of the countries discussed here, there is no systematic trend in the percentage remaining single at age fifty. Averages for each country even out a number of opposing changes, with a general tendency toward more regional homogeneity.

Maps 1 and 2 are meant to provide some insight into the regional differences in age at marriage and in the prevalence of permanent celibacy around 1870, a time close to the beginning of the fertility decline in three of the countries. France, where age at marriage was earlier and more women married, was well ahead in the transition to low fertility by 1866. The country had its share of areas with a late age at marriage—Brittany, the Rhine *départements*, and so on—but it also had many regions with what was, for Europe, an exceptionally low age at first marriage: an average of less than twenty-four years for women.

That the proportion married was higher and marital fertility started to decline earlier in France leads back to the relationship between the two factors. In the French *départements*, there is an undeniable association between low marital fertility—presumably caused by an earlier and wider use of practices of family limitation—and an early age at first marriage and low proportion of old maids. The proportion married was greater in 1866 in those areas where birth control had been accepted earliest and most widely. Similar conclusions could be reached by considering Belgium and the Netherlands, and would apply also to dates other than 1866. (Switzerland is a special case because of cantonal legislation restricting marriage.)

Table 1 lists the levels of marital fertility in the ten French *départements* with the highest age at first marriage in juxtaposition to those *départements* with the lowest. The proportion of women never married is also correlated with marital fertility by areas: the higher the proportion of old maids, the higher the fertility of those who married.

It is tempting to accept the thesis that society imposed restrictions on marriage to limit the number of children per completed

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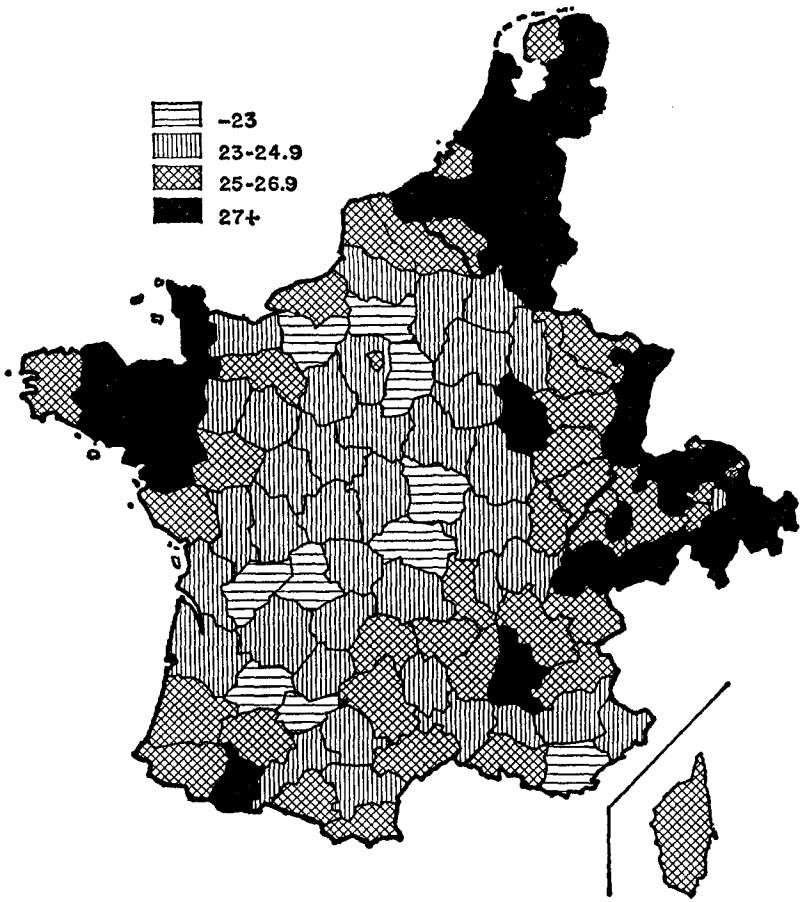
Map. 1

Mean Age at First Marriage. Women. France, Switzerland, Belgium, and the Netherlands, about 1870



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Map 2

Proportion Single at Age 50. Women. France, Switzerland, Belgium, and the Netherlands, about 1870

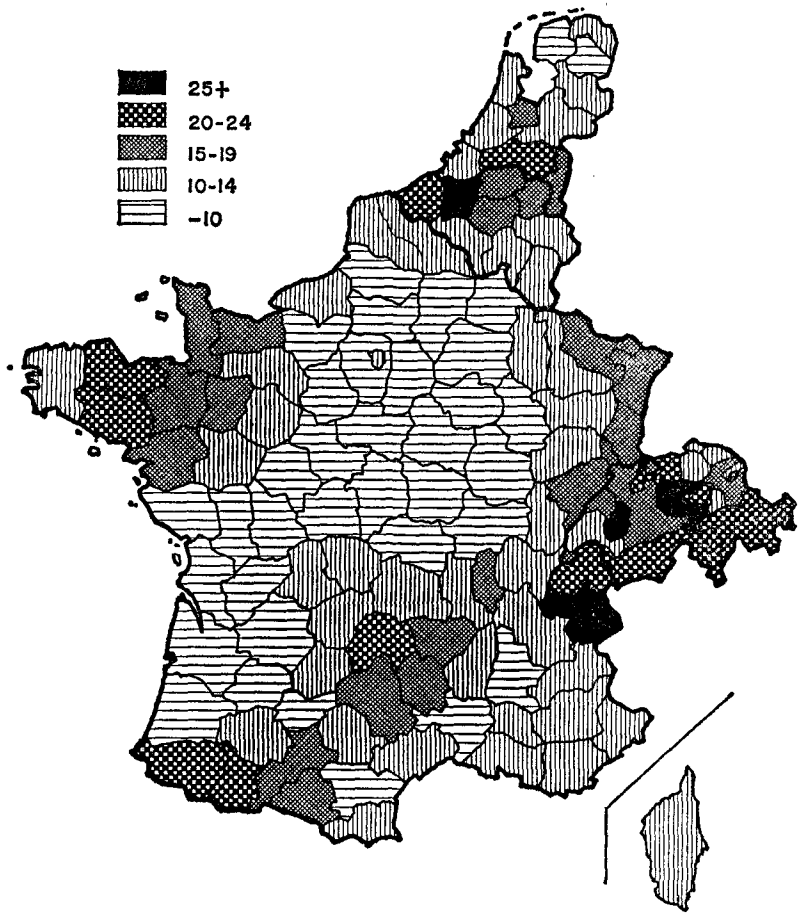

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Table 1

Marital Fertility (I_g) in Départements *with Highest and Lowest Age at First Marriage, France, 1866*

Highest Age at Marriage			Lowest Age at Marriage		
Département	Age at First Marriage in Years	I_g	*Département*	Age at First Marriage in Years	I_g
Morbihan	27.9	.737	Var	20.8	.440
Côtes-du-Nord	27.9	.802	Nièvre	21.5	.451
Haut-Rhin	27.9	.770	Haute-Vienne	21.6	.493
Drôme	27.8	.481	Lot-et-Garonne	21.9	.277
Haute-Savoie	27.7	.689	Seine-et-Marne	22.0	.385
Loire-Inférieure	27.5	.601	Oise	22.1	.350
Bas-Rhin	27.3	.774	Tarn-et-Garonne	22.4	.329
Hautes-Pyrénées	27.2	.513	Charente	22.7	.329
Ille-et-Vilaine	27.0	.679	Allier	22.8	.468
Manche	27.0	.521	Eure	22.8	.307
Average	27.5	.657	Average	22.1	.383

family and check rapid population growth. According to this interpretation, the advent of contraception released society from the need to impose such restrictions. A pattern of late marriage and diffusing contraception is thought to have served as a transition from the time when late marriage and uncontrolled fertility had been the rule to the present pattern of relatively early and general marriage with controlled fertility. If this interpretation is correct, the path followed from Strategy C to B ran through Strategy D.

A strong argument against this hypothesis is explicit in the evidence summarized in Figure 1. The data suggest that in both Belgium and the Netherlands age at marriage started to drop earlier than marital fertility. This also appears to be true for those areas of France and Switzerland where the decline in marital fertility began late enough to be documented by official statistics. Table 2 lists the age at first marriage in 1870 and 1900 for all areas which had not undergone a clear decline in marital fertility by 1900. In every instance, there is an important decline in the age at first marriage, which is obviously not the result of the adoption of fertility control.

Even if the facts were not clear, there would be *a priori* reasons to doubt the validity of an explanation that attributes a conscious desire to marry earlier to the diffusion of contraceptive practices. Although the origins of the European pattern of marriage are

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Table 2

Marital Fertility (I_g) and Age at First Marriage in Areas not Having Experienced a Decline in Marital Fertility by 1900. Switzerland, Belgium, and the Netherlands, About 1870 and 1900

Country and canton or province	I_g 1870	I_g 1900	Age at first marriage in years 1870	Age at first marriage in years 1900
Switzerland				
Freiburg	.816	.861	27.6	25.6
Ticino	.674	.682	27.2	25.7
Valais	.770	.789	27.1	25.6
Belgium				
Limburg	.866	.881	28.7	27.0
Netherlands				
Nd. Brabant	.931	.954	29.1	27.6
Overijssel	.786	.755	27.1	26.2
Drenthe	.750	.757	27.1	25.5
Limburg	.897	.968	28.3	27.7

obscure, we have no reason to assume that it was consciously intended to reduce the number of births since high fertility was universally valued as the primary aspect of a woman's role at this time. Etienne de Sénancour, the first writer known to tolerate contraception outside of marriage, has the following definition of love: "To love is to make a choice of the person with whom, according to certain affinities, one hopes to have happily constituted children, morally endowed with precious faculties."[18] He claims elsewhere that no woman would have any reason to divorce a man "as long as he renders her continuously fertile."[19] If the purpose and function of late marriage and widespread celibacy were to limit the birth rate, it is difficult to see why writers before Malthus so universally approved of high fertility. Malthus's use of the expression "preventive check" to population growth to characterize the moral restraint of those marrying late or rejecting marriage may have misled us into believing that there ever was an intent to prevent. That the rate of growth was actually slowed down is not, of course, in question. The existence of restrictions to early and general marriage and the use of contraception are essentially different in intent; the one is no substitute for the other. As a rule, contraception is accepted or practiced efficiently by couples at a rather late stage in married life, after the desired family size has been reached. Late marriage would, in many respects, be a method of avoiding or postponing a first birth only. It is unrealistic,

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perhaps, to suppose that engaged youths adjust the date of their marriage so that no excessive parity will be reached later in their lives.

Marital fertility apparently started to decline in many cases after the incidence of marriage had increased. Although the two events appear to be related (as was shown by Table 1), is this relationship necessarily one of cause and effect? We must be wary of the *post hoc, ergo propter hoc* fallacy. It is more likely that age at marriage and marital fertility started to come down because of the same economic or social causes. But what were these causes? We know that age at marriage fluctuated in the short run under the influence of economic circumstances, crises, and so on. The trend toward lower marital fertility, however, is generally linear from the pre-decline plateau until World War II; wars and crises hardly interrupt it.

The European pattern of marriage is contrary to human inclination. In Malthus's words, "the dictate of nature and virtue seems to be an early attachment to one woman."[20] As in other cultures, the normal tendency must have been to marry as early as possible. The origin of the European pattern must be left to speculation. Because it was oppressive, however, it is not difficult to explain a tendency to relinquish it. Economic constraints of various sorts discouraged early marriage. The general improvement in economic conditions during the nineteenth and twentieth centuries in Belgium, the Netherlands, Switzerland, and France made possible a lowering of the age at marriage. Under conditions of uncontrolled fertility, a decline in age at marriage would have had unfavorable economic consequences—as Malthus's reasoning made clear. Thus, the continuation of the decline for a long period necessitated a change in the attitude toward fertility control. Had the latter not come about, the natural tendency toward earlier marriage would have been frustrated. In this view, the decline in age of marriage was not provoked by a decline in marital fertility; the latter was a permissive agent without which the nuptiality trends would not have persisted in the long term.

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PAUL DEMENY

Early Fertility Decline in Austria-Hungary: A Lesson in Demographic Transition

"In traditional societies, fertility and mortality are high. In modern societies, fertility and mortality are low. In between, there is demographic transition." These propositions connecting basic demographic phenomena with the degree of modernity are neither subtle nor precise. Few students of population would be satisfied with this definition of demographic transition. Yet, with all their ambiguities, these three sentences unquestionably describe the central preoccupation of modern demography. Modern demography is, above all, about demographic transition. This is to be understood in two distinct, but interrelated senses. First, there is description in the spirit of the narrow, literal definition of demography—a description of quantitative aspects of human populations. Thus, contemporary demographers often ask: What is the level of fertility in traditional societies? When does fertility decline start? Where does fertility decline start? What is the speed of decline? What is the level of fertility in modern societies? On a more ambitious level, there is the task of explanation. The answers to each question about demographic facts are rephrased into questions. Why did fertility start to decline where it did? Why was the decline faster here than it was elsewhere? And so on. It is hoped that the answers to these latter questions will eventually jell into a *theory* of demographic transition: a set of generalizations which are capable of explaining the onset, the course, and the final outcome of past demographic transitions, and which will also give us a key to the prediction of transitions yet to come.

Why transition, as description or as theory, should occupy the center of the stage in modern demography is not difficult to understand. It would suffice in the present context merely to refer to the

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sheer historical interest in the topic. The demographic transformations that have occurred in the now modernized countries of the world did manifestly result in historically unprecedented patterns of reproduction, length of life, age distribution, and population growth that deeply altered the lives and involved the participation of virtually every individual member of the societies which were affected. One might assume that students of history would be eager to chart and understand a process of such dimensions and to trace its presumably multifarious economic, social, and psychological consequences. But historians, with all their propensity to clarify every obscure nook and corner of the past, have neglected the study of demographic processes to an extraordinary degree. The paucity of data required for such a study and the technical difficulties of interpreting the usually rather obvious deficiencies of existent numerical information go a long way to explain (but not absolve) the historians' neglect. It is to be hoped that historians and workers in other related disciplines will eventually acquire sufficient familiarity with demographic analysis and that the present unsatisfactory state of affairs will consequently change. In the meantime, demographers, a relatively new breed among social scientists, must try to fill the gap left by the more established disciplines concerning our knowledge of past population transitions and to erect a theory on the basis of such knowledge.

The task is evidently a worthwhile one, even if judged only on its intrinsic intellectual merit. Beyond that, conditions in the present-day world seem to make the task urgent indeed. The non-autochthonous character of the process of modernization presently revolutionizing the economically backward two thirds of the world has produced a nearly general and precipitous decline of mortality. This decline, in combination with especially high levels of fertility as established in the pre-modern era, has resulted in what nowadays is referred to, awkwardly but not without some justification, as the population explosion. The outcome of the explosion, quite obviously, depends on the future behavior of fertility. Apart from a few instances, relating to small or isolated populations, fertility in the less-developed countries has shown as yet no sign of an incipient decline. It can be demonstrated, however, that sustained maintenance of high fertility could jeopardize the process of economic and social modernization and, with it, the already achieved lower levels of mortality. Thus, more than ever, a theory of demographic transition is needed not only to explain the past and predict the

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future, but to guide in formulating rational and effective policies through which a decline of fertility could be induced artificially.

It would, of course, be naïve to assume that a theory specific enough to serve as a guide for policy could result from historical investigations alone. Differences between conditions in the Third World today and those in the countries which have undergone demographic modernization in the past are too deep-going and numerous for that. Nevertheless, history could provide some highly useful insights into the hierarchy of forces presently at work in shaping the levels of fertility. It might even suggest some rules for action if only by disproving certain assumptions that otherwise would lead to erroneous or at least unproductive policies. At any rate, it is difficult to say how useful history is for the present or for the future until one tries to understand the past. There can be no doubt that we have not yet tried hard enough.

The present essay examines the circumstances and certain salient features of the decline of fertility in the area commonly known with reference to pre-World War I Europe as Austria-Hungary, insofar as such an analysis appears to be of direct relevance to problems prominent in contemporary theorizing on demographic transition. The main limitations implicit in so circumscribing the scope of our discussion should perhaps be stated explicitly. First, no comprehensive nor balanced description of the Austro-Hungarian demographic transition will be offered. The focus will be on problems, rather than on the particular area. Second, the range of the problems discussed will be limited both by the points of emphasis in contemporary interest and by the nature of the area selected as the demographic laboratory—in this instance Austria-Hungary. Finally, our attention will be further restricted to the *beginnings* of the fertility decline, the perennial *pièce de résistance* of any theory of demographic transition.

A number of characteristics make the now long-defunct Hapsburg Empire of special interest as a demographic test tube for students of fertility transition. Two of these characteristics should be stressed here. The first is the population's exceptional diversity. The Hapsburg lands lacked many things, but were rich in ethnic, religious, cultural, and economic heterogeneity. Living side by side, peacefully or otherwise, in what was rightly called a multinational empire were Germans, Hungarians, Czechs, Slovenes, Italians, and Croats; Poles, Ukranians, Slovaks, Serbs, and Rumanians. The list of major religious denominations is almost as long as the list of

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the various ethnic groups. Contrasts among regions in terms of levels of literacy, degrees of urbanization, occupational structure, and other characteristics were always marked and remained so until the country was broken up into its constituent parts.

The potential usefulness of such heterogeneity in aiding demographic research should be obvious. Moreover, this potential can be realized because large masses of statistics of good quality and of easy comparability are available—an asset, unlike diversity, which was largely lost after 1918. This is not meant to imply that the whole region was under a unified statistical administration during the Hapsburg era. After the 1867 Compromise, separate organizations were set up for the Austrian and Hungarian parts of the dual monarchy. Diversity within each part was, however, almost as great as within the Empire itself. Furthermore, the two systems supplied data collected and tabulated on the basis of closely similar methods and principles.

Naturally, praise for the quality and quantity of data that exist for Austria-Hungary is to be understood in a relative sense only. In many respects, and particularly for the least developed regions of the country, the statistics are truly unique; elsewhere in Europe data of similar standards became available only at a substantially more advanced level of economic and social development. Nonetheless, even the best data are seldom of the sort the researcher would wish to have, as is always the case with historical statistics. Moreover, as one pushes backward in time, difficulties with the data increase rapidly, from the standpoint both of quality and of quantity. This imposes a final limitation on the discussion that follows. The problems of interpreting the inadequate data that originate from Austria-Hungary prior to 1880 and have a bearing on our topic are in some cases still unsolved. The use of these data would necessitate the discussion of intricate issues of data adjustment and interpretation that would go far beyond the necessarily narrow confines of this paper. Accordingly, data will be used here only when statistical tests have conclusively shown them to be substantially exempt from biases due to omission or misreporting.

A Digression on Demographic Indices

In our analysis, we seek to describe and compare levels and changes in fertility. For this purpose, we need an index that measures over-all fertility—that is, the reproductive performance of

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women of childbearing age. It is obvious, however, that in populations where procreation takes place primarily within marriage, we should consider separately the two immediate factors that determine over-all fertility performance—namely the fertility of the *married* women, on the one hand, and the *proportion married* among all women of childbearing age, on the other. Thus, our discussion will run mainly in terms of three indices: the index of over-all fertility, denoted as I_f; the index of marital fertility, denoted as I_g; and the index of the proportion married, denoted as I_m.

The specific method of calculating these indices is somewhat laborious and will not be described here.[1] Fortunately, the essential meaning of the indices is sufficiently described by their names, and only a few comments need to be added here to facilitate their interpretation. The value of each index may range from a low of 0 to a high of 1. Thus, for example, I_g assumes a value of 1 if marital fertility is at its theoretical maximum where married women bear children at the highest rate ever recorded among populations known to have "natural" fertility (practicing no birth control). This amounts to taking as a standard the marital fertility that characterized women in the 1920's among the Hutterites (a small North American population) and expressing marital fertility in terms of that standard. Accordingly, a value of I_g of .8, for example, indicates that the number of children born in a given year is 20 per cent less than would be the case had married women experienced a fertility at each age as high as in the standard (Hutterite) schedule. (It is assumed in these calculations that illegitimate fertility is zero.) In an analogous fashion, an over-all fertility index (I_f) of .3 indicates that the number of children in a given year falls below the theoretical maximum by 70 per cent. That maximum would be attained if all women, regardless of marital status, would bear children at the rate indicated by the standard schedule. It should be noted, finally, that there is a simple arithmetic relation between the three measures, the index of over-all fertility being a product of the indices of marital fertility and of the proportion married. In other words $I_f = I_g \times I_m$.

The relationship just noted permits us to represent all three indices in a simple two-dimensional diagram (see Figure 1). Once the reader has familiarized himself with this diagram, it conveys information in a highly illuminating and concise manner about the level of the indices at a given moment as well as about changes over time. By the same token, the graph facilitates comparisons

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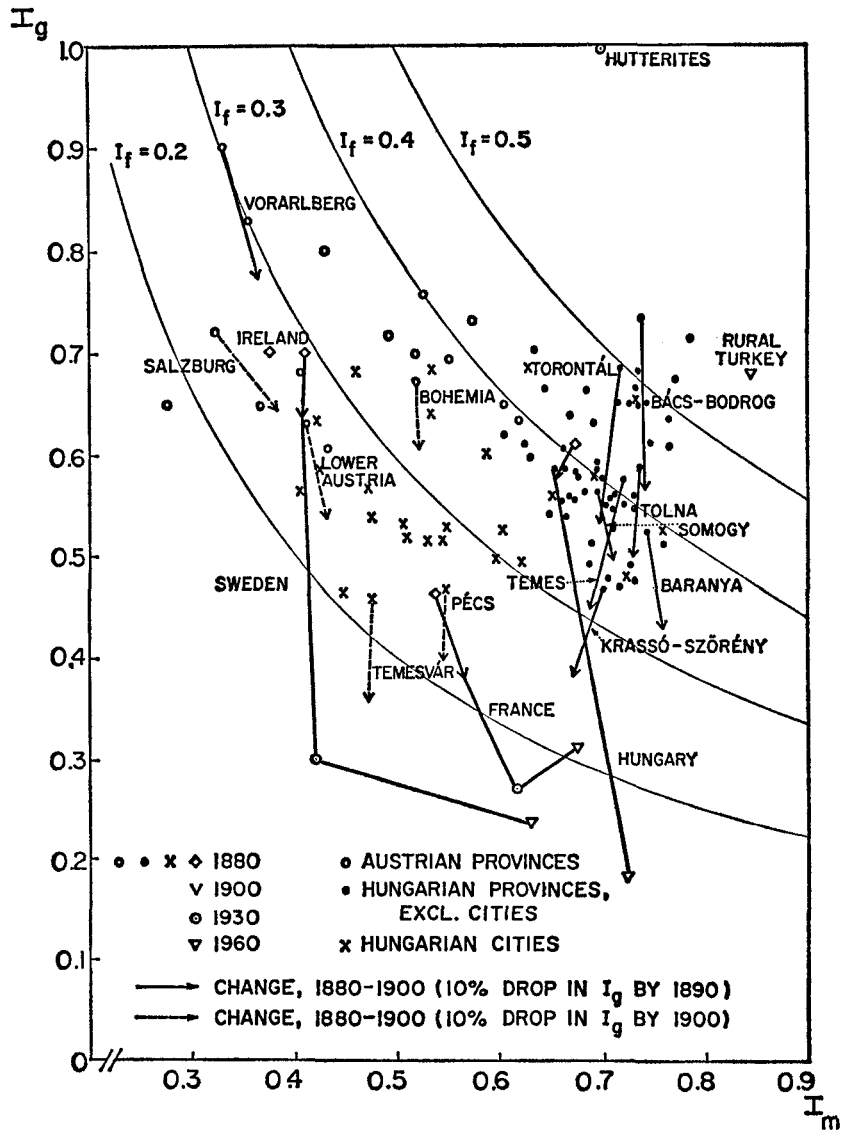
Figure 1

Indices of Marital Fertility (I_g) and Proportions Married (I_m)



among indices of various regions and among patterns of change.

The levels of marital fertility (I_g) are measured along the vertical axis of Figure 1. Distances along the horizontal axis indicate

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the value of the index of proportions married (I_m). Since over-all fertility equals $I_g \times I_m$, lines along which the value of I_f is constant are also easily represented on the diagram. Four such "isoquants" of I_f are drawn on Figure 1; with reference to these, the approximate level of over-all fertility is easily determined at a glance.

Fertility Around 1880 and After: An Overview

Statistics providing a cross-classification of marital status with age and sex are not available for Austria-Hungary until the Census of 1880. Thus, calculation of the indices (and, for that matter, even calculation of simpler measures that permit the crucial factoring of fertility into marital fertility and proportions married) is feasible only for the years around 1880 at the earliest. The results of such a calculation are given in Figure 1. Specifically, the figure shows the position, around 1880, of the seventeen Austrian provinces (*Länder*) as well as the position of the more than sixty Hungarian provinces (excluding Croatia-Slavonia). For Hungary, it is possible also to separate the cities (defined by administrative criteria) and the non-urban (rural) areas. Accordingly, the values for the cities of Hungary are indicated separately, and the figures for the provinces (*vármegye*) do not include the urban segments, if any.

If one then calculates the indices for another date—for example, for the years around the 1890 census—one can plot another set of points on the map. By connecting with an arrow the earlier point with the later one for each province, one has a visual image of the change in fertility (and nuptiality) during the decade. This procedure can be repeated—moving on to 1900, to 1910, and so on. For each population, the series of dated points interconnected by arrows describes the path that I_m, I_g, and I_f traveled over time. Since our interest is focused only on the early decline of fertility, we need not overburden the diagram by representing the change for every province and for a long span of time. We limit our attention to the leaders and to the last two decades of the nineteenth century. Accordingly, we have used 1880 as a base and have selected all provinces where marital fertility, as measured by I_g, had declined by at least 10 per cent by 1890. There are eight such provinces: one in Austria and seven in Hungary. Four of the Hungarian cities also showed a similar decline. So that we would not rely on developments in a single decade only in detecting early

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fertility decline, we have also selected from among the remaining provinces all those in which a 10 per cent decline occurred by the turn of the century, again in relation to the 1880 level. Three Austrian provinces, but none of the Hungarian ones, have satisfied this requirement. Some seven more of the Hungarian cities, however, did witness a substantial decline by 1900. The changes in the indices between 1880 and 1900 in the Austrian and Hungarian provinces selected by the criteria just described are represented in Figure 1 by arrows: the head of the arrow indicating the position in 1900. A solid line is drawn if a marked decline has occurred by 1890; a broken line indicates that at least a 10 per cent drop relative to 1880 was registered by 1900. None of the provinces for which only the 1880 position is indicated in the diagram showed a similar decline by the turn of the century. As to the cities, the 1880-1900 change is shown only for the two that had the lowest I_g values by 1900 in absolute terms.

In order to put into perspective the 1880 levels and the declines in the subsequent twenty years shown for the Austro-Hungarian provinces, Figure 1 also gives the location of the indices for some other populations that may be considered extreme in some sense. The Hutterites exemplify populations with the highest level of marital fertility. Late-nineteenth century Ireland is known for its extreme restriction of marriage. In contemporary rural Turkey, marriage is early and nearly universal, and fertility of marriages is presumed to be very high. The Hungary of the early 1960's is described by an I_g value that is probably the lowest ever experienced in any sizable national unit. Finally, also for purposes of comparison, Figure 1 shows three notable patterns of demographic transition: those for Sweden, France, and Hungary (estimated for the present national frontiers). The transitions are traced from 1880 on, first for the same period as given for the Austro-Hungarian provinces (1880-1900) and then in a deliberately schematic manner up to around 1960.

Pre-Decline Levels of Marital Fertility

We can now proceed quickly with an examination of the demographic facts summarized in Figure 1. The first question that may be asked concerns the level of pre-decline fertility. Let us assume for the moment that the pre-decline situation in Austria-Hungary is the one described by the indices for around 1880. A glance at

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Figure 1 shows that differences in the over-all level of fertility were large, ranging from a value of below .2 to well over .5—that legitimate births in the Austro-Hungarian provinces amounted roughly to some 20 to 50 per cent of the potential maximum. Such marked differences were a result partly of the differences in I_m and partly of the differences in I_g. Let us consider the latter factor first.

Any reasonable description of fertility transition contains the idea that marital fertility is "high" before transition, but "low" after transition. Ordinarily, low marital fertility is the consequence of deliberate control of what demographers call "natural" fertility. Couples presumably exercise such control so as to insure that the number of children born to them conforms to their preferences concerning size of family. Since preferences may differ considerably from population to population, a great deal of variability of "low" marital fertility may result even if contraception is universally and effectively practiced. On the other hand, the notion of "high" marital fertility implies the absence of deliberate interference with natural fertility; hence, one would expect a substantial degree of uniformity within populations having pre-decline marital fertilities. In the Austria-Hungary of the late-nineteenth century, not only is such uniformity missing, but the quantitative differences among the I_g values for the various provinces were extraordinarily large. To be specific, values ranged from a high of over .9 to a low of about .46.

Such differences in pre-decline marital fertilities constitute an unsolved demographic puzzle of the first order. One must add quickly that no solution of the puzzle is offered here. We shall limit ourselves simply to stating two alternative lines along which an explanation of the facts may be attempted. The first of these would maintain the assumption that no birth control was exercised by individual couples for the express purpose of limiting family size. The burden of explaining differences in I_g would then fall on differences among populations with respect to exposure of women to intercourse, to the capacity to conceive (fecundity), and to the probability that a pregnancy is terminated by a live birth.[2] These variables are, in turn, affected by such factors as racial-biological characteristics, standards of general health and nutrition, length of lactation, and a variety of social customs and regulations.

It is, of course, beyond dispute that significant differences in the level of natural fertility may and do exist. For example, one would certainly shy away from asserting that birth control was practiced

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in the province of Vorarlberg because its marital fertility was some 10 per cent below the Hutterite level, even though such an interpretation may well be correct. On the other hand, differences of the sort shown in Figure 1 can hardly be attributed to differences in natural fertility alone. Ideally to support this assertion, more detailed data on at least the actual fertility performance would be necessary —such as, for example, individual fertility histories, parity distributions, or age-specific fertility rates. Lacking data of this sort, one may study differences of marital fertility among provinces and, in some instances where this is feasible, among smaller subdivisions of individual provinces that are closely similar with respect to the characteristics that presumably affect natural fertility (health, nutrition, social customs, and so forth). Our investigations for Austria-Hungary have shown that I_g levels among such populations are still apt to differ greatly, thus weakening, even if not conclusively destroying, the plausibility of the argument that natural fertility differences alone are responsible for the disparities in I_g. When an argument seeks to explain as much as is sought to be explained in this instance, however, the burden of proof is on those who have offered the argument in the first place.

This brings us to the alternative hypothesis that could explain the differences in marital fertility shown in Figure 1 in a much simpler fashion—namely, by assuming that such differences constitute *prima facie* evidence of some form or forms of deliberate birth control within marriage. According to this view, such control must have been exercised by at least an appreciable fraction of the married population in most, if not all, of the Austro-Hungarian provinces around 1880, and presumably for a long time before that date. Unfortunately, as so often happens in historical demography, this conclusion is based essentially on deductive reasoning and cannot be regarded as definitely proved, even though some impressionistic and literary evidence indicates that primitive techniques of induced abortion were indeed known and practiced in certain parts of Hungary, and that various folk methods of contraception, including crude intrauterine devices, had considerable currency.

Should the reader find the postulate of pre-decline birth control an easy one to accept, let him be reminded that it is in sharp conflict with much of received theorizing on demographic transition.[3] If we admit that birth control of one form or another had been practiced *before* the secular decline started, a central feature of transition theory is erroneous—the vision of the onset of fertility

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decline as an instance of social mutation, as a clear-cut qualitative change in behavior. Thus, the decline of fertility would have to be viewed as the result of a merely quantitative change in the frequency of age-old practices that presumably came about as a natural response to changing environmental pressures. The implications of such a reinterpretation of demographic history are far-reaching, notably with respect to formulation of population policy in present-day high-fertility countries. Although their discussion is beyond the confines of this paper, a possible objection to the above argument may be considered briefly.

The preceding discussion assumed that the 1880 indices do, in fact, represent the pre-decline situation in Austria-Hungary. Is this a reasonable assumption? On the face of it, it certainly is, since we know that, apart from the notable exception of France, the downturn of fertility occurred in Western Europe only after 1875. If anything, one would expect that this event came later in Austria-Hungary. Nevertheless, the choice of 1880 as a date of reference was not dictated by such considerations: It was simply imposed on us by the limitations of the available data. Could it be, after all, that the decline in Austria-Hungary started well before 1880 and that the large differences in I_g found around this date simply reflect the result of developments prior to 1880 which brought down marital fertility from more or less identical levels, but at differential rates of change?

There is plenty of statistical evidence that bars the assumption of an appreciable pre-1880 decline as far as Austria is concerned. For Hungary, the pre-1880 record is much less clear, partly because of the deficiencies of the underlying census and vital statistics and partly because correction for such deficiencies is exceedingly difficult since certain genuine demographic disturbances affected the population during that period, notably those associated with the devastating cholera epidemic of the early 1870's. Nevertheless, there *is* evidence of a relatively small but noticeable fertility decline in a few provinces; indeed, in some segments of certain provincial populations, the downturn can be traced to the 1850's. Not surprisingly, the provinces that experienced a decline by 1880 are among those marked in Figure 1 as showing a continuing drop. Since these provinces also had low I_g's in 1880, some of the extreme marital fertility differences found for that date are, in effect, artifacts of previous downward changes in marital fertility. Nevertheless, one can find no apparent explanation in earlier declines for

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the mass of provincial differentials registered around 1880. The dynamics of I_g's *after* 1880 appear to support this contention. As was pointed out earlier, the large majority of the Hungarian provinces exhibited no substantial decline of marital fertility by 1900. Decline was appreciable and widespread after the turn of the century, but even by 1910 a good number of provinces (located mostly in Transylvania) had witnessed no decline in marital fertility relative to the level in 1880 (at any rate, none detectable by a cross-sectional index, such as I_g). Notably, the provinces that showed an almost perfectly horizontal plateau of I_g between 1880 and 1910 are the very provinces which had low, although not the lowest, marital fertility in 1880. To assume a pre-1880 decline from high fertility to such levels—to I_g's ranging from .5 to .6—and then a stabilization for at least some thirty years at that level would be not only farfetched, but would raise more problems than it would help to solve. Moreover, analysis of the age distributions in these provinces indicates that the 1880-1910 plateau represents a fertility level that prevailed during the indefinite period *prior* to 1880 as well. Thus, the existence of large pre-decline differentials of marital fertility stands unshaken and with it the remarkable, though still tentative, conclusion that points to extensive interference with reproduction within marriage in populations heretofore considered, explicitly or implicitly, as having "natural" marital fertilities.

Patterns of Nuptiality and Fertility Decline

The contrasts we have found among provincial I_g values in Austria-Hungary around 1880 are fully matched and even exceeded by differences in the index of the proportions married relating to the same date. As Figure 1 shows, a few of the Austrian provinces exhibit I_m values of .35 or lower, the extreme being an almost incredible .28 registered for Carinthia. At the other end of the scale, the rural populations in almost all of the Hungarian provinces have I_m values of over .65; the majority of provinces have I_m's exceeding .7; some provinces have I_m's exceeding .75.

Such differences are, of course, much less surprising than differences in pre-decline marital fertility. It has always been recognized that control of fertility through the classic Malthusian method —restriction of marriage—is practiced in all societies, but at greatly differing degrees. Historically, Western Europe constituted a case

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sui generis, having adopted a nuptiality pattern characterized by high average age at marriage as well as by high proportions remaining single throughout the childbearing period. As was recently pointed out by John Hajnal,[4] Austria-Hungary straddled the boundary delineating the area displaying this Western European pattern. Naturally, the division is not so sharp as summary comparisons would suggest, as is indicated by the distribution of points in Figure 1 with respect to the I_m scale. Even if the largely intermediate-type city figures are ignored, the provincial data still show some gradation between extremes. This point would be even stronger were some large Austrian provinces, notably Galicia, broken up into more homogeneous geographical units. Nevertheless, the contrasts are clear. In Hungary, around 1880, average age at first marriage was typically twenty-one or twenty-two years for women, and the proportions remaining single seldom exceeded 5 per cent. On the other hand, the average age at first marriage for women was above twenty-seven years in all of the predominately German-speaking provinces (in one instance, going as high as 28.3 years), and the proportions remaining single typically exceeded 20 or even 30 per cent (the extreme case being Carinthia's figure of 45 per cent).

For the present purposes, our interest in these striking differences is limited to their relations to the level and decline of marital fertility. Figure 1 shows that there is a somewhat blurred, yet unmistakable, negative relation between I_m and I_g, particularly if the cities are not considered. In other words, with respect to over-all fertility, proportions married and marital fertility pull in opposite directions. Accordingly, it may be suggested that, to a certain extent, restriction of marriage and restriction of births within marriage are employed as alternative tools in achieving the goal of restricting over-all fertility. While this seems to be a reasonable generalization, it is important to emphasize that the various provinces seem to differ merely with respect to the specific mixture in which they employed the tools of control—that is to say, at any given level of I_m, marital fertility shows a large enough variation to make the discussion in the preceding section applicable. Even in the provinces with low I_m values, there is clear indication of successful limitation of births within marriage. It would seem, therefore, that once modernization upsets the pre-decline equilibrium of fertility, the decline should start from essentially similar conditions with respect to tested knowledge of birth-control

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methods, regardless of the exact initial levels of I_m and I_g.

Nevertheless, upon further reflection, it would be reasonable to expect that the quantitative mix of the pre-decline controls on over-all fertility should have an important bearing at least on the speed of decline in marital fertility. Consider, for instance, a situation where identical levels of over-all fertility are maintained, but through different combinations of I_g and I_m. This obviously means that the population with the higher proportions married must be characterized by a higher average level of practice (and perhaps of knowledge and acceptance) of birth control or by a higher proportion of families practicing control. In such a population, a newly generated pressure favoring low fertility would presumably result in an earlier and faster decline than would the same pressure in a population with the same over-all fertility, but with a higher I_g and a lower I_m. Our inability to find provinces satisfying the necessary *ceteris paribus* clause, of course, prevents us from testing such a proposition. Nevertheless, the deduction appears to be roughly consistent with the early substantial decline that occurred in those provinces of the country in which the pre-decline fertility plateau was the result of high I_m's and of low I_g's. The single exception to this is Vorarlberg: the province with the highest single I_g value. Paradoxically, that decline may be explained by the anomaly of a disproportionately high over-all fertility as compared to provinces with identical proportions married. The process was apparently one of adjustment and convergence to the norms prevailing in the immediate geographical neighborhood.

It should be admitted, however, that *within* the group of provinces described by early and quasi-universal marriage (that is by high I_m's) the level of over-all fertility appears to have no obvious relation to the onset and speed of early decline. Still, the pre-decline levels of over-all fertility for these provinces, whether taken as a group or individually, were clearly higher than in the provinces with heavy restrictions on marriage. Thus, in the former group, not only did contraception have to be more widespread, but absolute demographic pressure was also probably more pronounced. This observation should further lessen our surprise that the early declines were registered mostly among provinces with high I_m's and low I_g's.

Thus far, we have tacitly assumed that the process of fertility transition involves merely changes in I_g, while I_m remains on the level established in the pre-decline equilibrium situation, at least

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during the early transition. It is, indeed, tempting to postulate two broad historical patterns of fertility transition that are consistent with such an assumption of early stability in I_m (as illustrated in Figure 1 by Sweden, on the one hand, and Hungary [post-World War I territory], on the other). The first is a two-phase transition of a characteristically Western European type. In this pattern, I_g drops steadily during the initial phase without any appreciable change in I_m, whose absolute level is low. Once over-all fertility is drastically reduced by highly effective control of fertility within marriage, the natural tendency to marry, and to marry early, is released and I_m increases; nuptiality ultimately loses its distinctive Western European pattern. The second type of transition starts from the condition of early and quasi-universal marriage. The fertility transition is dominated by, and largely limited to, a decline in marital fertility. The pattern of nuptiality is preserved throughout the process in its essentials, and the post-decline equilibrium of nuptiality is the same as the one established in pre-modern times, perhaps with some tendency toward even higher proportions married.

An examination of the detailed record of the Austro-Hungarian transition suggests that these models should not be taken too literally. As far as the early stage is concerned, Figure 1 tends to indicate that some element of convergence in marriage patterns was present during the last decades of the nineteenth century. In provinces where I_m was initially low, it had a tendency to increase and vice versa. This finding would at first seem to be highly significant since it would suggest that nuptiality has played different roles in the transition—reinforcing the transition toward low fertility where it was high to begin with, and militating against fertility reduction where it was low initially.

A closer analysis of the data, however, discourages such interpretations. In particular, changes in age at marriage and proportions married showed only minor variations during the early phase of the transition in the provinces with a high I_m; moreover, these were short-run variations quickly reversed and not systematically related to the drop in I_g. For the low-I_m (Austrian) provinces, the record is somewhat more ambiguous. Without question, there was a noticeable drop, particularly in the German-speaking provinces after 1880, in the age at marriage and an even more pronounced increase in the proportions eventually marrying. These changes are well illustrated by the following data for women:

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Province	Index of Proportion Married		Mean Age at First Marriage		Proportion Single Around Age 50	
	1880	1910	1880	1910	1880	1910
Lower Austria	.412	.440	27.8	26.8	.155	.168
Upper Austria	.407	.436	27.8	27.2	.223	.166
Salzburg	.323	.400	27.2	26.4	.389	.232
Styria	.366	.416	27.3	27.1	.300	.208
Carinthia	.276	.372	27.9	26.0	.447	.319
Tyrol	.350	.413	28.0	26.4	.291	.238
Vorarlberg	.330	.385	28.3	26.9	.317	.248

This, of course, is far from a static situation. Since the changes were well under way before marital fertility started to drop (indeed, the trends were already operative before 1880), it may be assumed that an autonomous increase in the proportions married was an important factor in upsetting the pre-decline equilibrium. Such an increase must have led to higher over-all fertility, thus creating new demographic pressures and requiring countermeasures which, given the new and higher propensity to marry, were necessarily controls *within* marriage. Indeed, it is possible that a change in marriage patterns may be credited with a catalytic role in bringing about a decline of marital fertility in the Austrian part of the Hapsburg Empire. The quantitative significance of these changes must, however, be kept constantly in mind. In comparison with the impact of changes in marital fertility, it was decidedly minor. Once marital fertility started to drop, its impact went quickly beyond any reasonable compensation for earlier marriage. As the limited sample in Figure 1 indicates, the effect of I_m's increase was far overshadowed by the effect of I_g's drop, even by the end of the initial period of fertility decline. All in all, even the German-speaking Austrian provinces can be said to have followed fairly closely the pattern exemplified by Sweden. The significant step toward earlier and general marriage had to wait until control of births within marriage brought over-all fertility well below the level needed for the sheer reproduction of the population from one generation to another.

Factors in Early Fertility Decline

Apart from the demographic characteristics of the pre-decline

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situation narrowly defined in terms of indices of fertility and nuptiality, what factors help to explain why fertility declined early in some areas of Austria-Hungary and not in others?[5]

First of all, an outline of the geographical pattern of early decline is in order. The only Austrian land achieving a substantial drop in marital fertility by 1890 was Vorarlberg, at the westernmost end of the Empire. As was suggested above, an early decline in this small Alpine province, wedged between Tyrol and Switzerland, is not particularly surprising in view of the exceptionally high level of the pre-decline I_g that was bound to become anachronistic as the population's contacts with the outside world multiplied. In this sense, the province was a follower, rather than a leader. The real centers from which modern fertility transition spread in Austria were the provinces reducing I_g appreciably only in the last decade of the century—in particular, Lower Austria and Bohemia. In the present context, Lower Austria should read simply "Vienna," the capital city which is also the center of the province. In the rural areas of Lower Austria, fertility did not change until after 1900. Early decline was more general in Bohemia (the Western third of present-day Czechoslovakia), which was the most advanced region of the Empire economically. Thus, in the Austrian lands, fertility decline does seem to fit the conventional picture of demographic transition as a process associated with urbanization, industrialization, and their various correlates.

The same cannot be said, however, about the seven Hungarian provinces that constitute the important example of early fertility decline in the Hapsburg Monarchy. Remarkably, the seven provinces that met the test of early decline form an unbroken string, stretching along the north bank of the rivers Drava and Danube in what was then southern Hungary. The westernmost member of this group is the province of Somogy, south of Lake Balaton. Some three hundred miles to the east is the other end of the string with the province of Krassó-Szörény,[6] located south of the river Mures and north of the section of the Danube ending at the Iron Gate. The whole area, now divided among Hungary, Yugoslavia, and Rumania, contained somewhat over three million people around the turn of the century, a population that was and has remained predominantly agricultural. The lack of appreciable urban-industrial development in the area suggests that the single most important factor regularly invoked in explanations of transition is inapplicable here. In fact, it can be positively shown that the decline of fertility

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originated and developed in and among the peasantry. The few urban centers of the area were distinguished neither by especially low I_g's nor by especially fast declines of fertility. This is to be understood in relation to the surrounding rural areas. Fertility in Pécs or in Temesvár—the centers of provinces Baranya and Temes respectively—was below that in Vienna or Budapest. Krassó Szörény, in some respects the most backward among the seven, had by 1880 a level of marital fertility as low as that reached in Vienna twenty years later.

If the provinces are compared with other areas in Hungary with respect to such characteristics as infant mortality rates, proportions literate, or rates of in- and out-migration— elements believed to affect the timing of the onset of fertility decline—once again they show no distinguishing characteristics that could account for the story told by Figure 1. Nor is the story explainable in terms of some peculiarity of the area with respect to standards of health, nutrition, housing, climate, or topography.

Following the classic approaches, we thus seem to reach an impasse. Negative lessons are, of course, not entirely worthless; it can at least be said that late-nineteenth-century Hungary supplies a remarkable proof for a possible adoption of increasing degrees of voluntary control of marital fertility in the absence of any fundamental modernization of the underlying socio-economic structure. In a more positive vein, three points should finally be made. First, it can be shown that the process by which family limitation spread within the seven-province area was to a significant degree one of geographic diffusion. This is by no means a trivial proposition; there are few documented instances of such a process. Only too often geographical differences observed cross-sectionally at a given time are interpreted as evidence of diffusion when, in fact, the differences may be of a quite stable nature that persists unaffected even in a period of change. To some extent, this was the case in the area in question, but there is also clear evidence of transmission in the form of time lags determined by geographic location. The process of transmission need not be interpreted as a transmission of contraceptive knowledge. Our discussion of provincial differences in pre-decline levels of I_g suggests that it is unreasonable to assume the lack of such knowledge in practically all provinces of Austria-Hungary. If we carry the analysis to a sub-provincial level (examining, for example, differences among the several thousand villages located in the seven provinces), an analogous picture

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emerges. The variance of fertility is so large that with the possible exception of a relatively small number of villages the assumption of a state of natural fertility is highly questionable. If so, the diffusion observable within the provinces leading the fertility decline was presumably one of ideas, aspirations, and attitudes, rather than techniques. Conscious manipulation of exposure to outside contacts, thus, may affect the speed of fertility decline. At the same time, the presence of persistent earlier differentials within the area shows that such exposure is insufficient in itself to induce a change in behavior not otherwise desired.

Second, although the ultimate chemistry of the agent that makes people *prefer* smaller families remains elusive, the description of the groups that moved in this direction is instructive. By isolating villages that are homogeneous with respect to certain statistical criteria and by observing trends and levels of fertility within such villages, we can isolate at least three "leading" groups. In the Transdanubian region, the nucleus of change is found among the Protestant (Calvinist) Hungarian population. In the Banat region, the leaders were Roman Catholic and German. Further to the east, in Krassó Szörény, early fertility decline is a characteristic of the Greek Orthodox Rumanian population. In the first instance, the relevant factor can be shown to be religion. There is a sharp contrast between Hungarians depending whether they are Protestant or Catholic, although the spread of birth control among Catholics in Transdanubia (in Baranya, Tolna, and Somogy) can be traced at least to the early 1890's, perhaps three decades after the Calvinist example. In the Banat and Krassó Szörény, the emphasis is on nationality: The Hungarians and Serbs of the region (Roman Catholics and Greek Orthodox respectively) were late in reducing marital fertility. The demographic behavior of the leading groups was extraordinary in many respects. By the first decade of the twentieth century, reproductive performance in a large number of villages was well under replacement levels, and birth rates sank below twenty or even fifteen per thousand, consistently remaining below the level of crude death rates. The evidence indicates that an increasing proportion of the families sought to raise only a single child.

To assert that such behavior is somehow associated with, say, living in southern Transdanubia, being a Hungarian, and being a Calvinist—all conditions to be satisfied simultaneously—is perhaps not overly edifying. A more significant lesson, however, lies in the

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demonstration that identical behavior was generated in a variety of greatly differing cultural environments. In each instance, this happened in the face of official disapproval, in the absence of modern methods of contraception, and without much exposure to any form of mass media. One must conclude that common forces were at work which transcended religion or race, but were triggered into unusually early action in certain cultural settings. Unfortunately, these fundamental factors are elusive, and it is difficult, if not impossible, to isolate them statistically. In the present instance, significant evidence points to the paramount importance of economic aspirations—specifically the desire to acquire, to hold on to, and to accumulate land under the increasingly adverse combination of growing population pressures, stable or expanding latifundia, and limited availability of alternative employment opportunities. More vaguely and speculatively, a number of features in the area's historic background may be pertinent to the early decline. It should be noted in this context that much of the land was repopulated during the eighteenth century as a result of organized settlement schemes, and for a long time, segments of the repopulated land were administered as part of the Military Frontier Region. Undoubtedly the fact that the area was a meeting point of at least four major nationalities and at least three major religions, unseparated by political boundaries, is not without relevance in itself.

Finally, it should be mentioned that the detailed analysis of intra-provincial differences reconfirms to some extent the importance of at least two factors emphasized by traditional transition theory: infant mortality and literacy. Although inspection of the average values of these indices does not single out the seven provinces as likely candidates for early fertility decline, *within* each province early decline does tend to show an association with both lower infant mortality and higher literacy. This proposition remains valid even when the disturbing influence of other correlated factors (notably nationality and religion) is eliminated. Once again, the role of these factors is facilitating and permissive, rather than decisive. Interestingly, the experience of the region in question shows that infant mortality rates that are very high by modern standards even for the less-developed countries (such as two hundred or more infant deaths per thousand live births) are quite consistent with drastic and increasing limitation of marital fertility.

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2. Compare Kingsley Davis and Judith Blake, "Social Structure and Fertility: An Analytic Framework," *Economic Development and Cultural Change,* Vol. 4, No. 4 (April, 1956), pp. 211-35.

3. See, however, Gösta Carlsson, "The Decline of Fertility: Innovation or Adjustment Process," *Population Studies,* Vol. 20, No. 2 (November, 1966), pp. 149-74. For a survey of the literature, excluding the most recent work, see Ronald Freedman, "The Sociology of Human Fertility," *Current Sociology,* Vol. 10/11, No. 2 (1961-62), pp. 35-121.

4. John Hajnal, "European Marriage Patterns in Historical Perspective," *Population in History,* eds. D. V. Glass and D. E. C. Eversley (London, 1965), pp. 101-143.

5. In view of the complexity of the socio-economic causes underlying the fertility transition, an answer to this question calls for the marshaling of large masses of information and for a detailed analysis, tasks that cannot be attempted in the present essay. As a conclusion to this paper, only a few descriptive comments will be offered which point to some of the factors involved.

6. Unless common English terms can be substituted, administrative divisions are referred to here with the names that appear in the official statistical publications of the period.

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MASSIMO LIVI-BACCI

Fertility and Population Growth in Spain in the Eighteenth and Nineteenth Centuries

DEMOGRAPHERS HAVE for some time been devoting their attention to the secular (long-term) decline of fertility in the Western populations—its determinants, concomitants, and consequences. Although a completely satisfactory theory explaining the decline has yet to be formulated, it is clear that the decline is closely related to the spread of birth control during the nineteenth and twentieth centuries. The increasing acceptance of a neo-Malthusian attitude toward procreation and, also, the voluntary limitation of family size within marriage are characteristic features of the demographic development of Western populations during the last hundred years.

In recent decades, the demographers' attention has turned increasingly to the problem of the nature of fertility in pre-modern populations. Their interest lies in charting what happened before the onset of the decline. Were birth-control techniques spread by the "diffusion" of already existing techniques practiced at least among certain "upper" sectors of the population or by genuine "innovation," involving modification of old models of behavior?

It has long been commonly held that populations do not experience substantial changes in the level of legitimate fertility before systematic and continuous decline in fertility. According to this assumption, modifications in the birth rate are attributable to changes in the frequency of marriage and illegitimacy or to shifts in the population's age-sex distribution. Deeper analysis, however, has shown that the levels of fertility may change because of the varying impact of sterility, pregnancy wastage, duration of breast feeding, and so forth. Thus, the birth rate may vary not only because of changes in a population's age-sex marital-status structure, but also because of the action of biological factors. If we follow Louis Henry's definition, pre-modern populations lived in a *régime*

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of natural fertility that was unaffected by volitional factors. Even such taboos as the abstention from sexual intercourse during lactation cannot be defined as a voluntary check on fertility.[1]

Nevertheless, it has not yet been demonstrated that biological factors account for all the differences in the levels of fertility among and within pre-modern populations. The conspicuous variations in fertility among populations, the strong rural-urban or class differentials within the same population, the notable diffusion of neo-Malthusian habits among selected (upper) sectors of the society, long-term changes in fertility within the same population (when controls are made for other factors) are elements that may support the theory that voluntary control of fertility was also at work in pre-Malthusian populations. This theory gathers further support when one examines the birth-control techniques employed before the large-scale diffusion of manufactured devices—techniques, such as *coitus interruptus,* that could be used by any sufficiently motivated couple.[2]

When more material is analyzed with the tools of modern demography, the distinction between "natural" and "controlled" fertility will probably become less clear cut. This distinction will remain useful, but on theoretical, rather than empirical, grounds.

General Characteristics of the Spanish Demographic Development

Official data for Spain are unreliable until the end of the nineteenth century, but they do indicate that fertility had begun to decline there by the turn of the century, two or three decades later than it had in most Western European countries. As in other European countries, however, recurrent ravages of "catastrophic" mortality affected population growth in Spain from the sixteenth century to the beginning of the eighteenth. As far as demographic development is concerned, the notable features of Spain's catastrophic century are the plagues of 1598-1602 and 1649-52, the expulsion of the Moriscos in 1609, the emigration to the New World, and the years of very high mortality caused by poor harvests. Various enumerations in 1591-94 put Spain's total population at 8.5 million; this figure is reduced to 7.5 million in 1717 and increased to 9.3 million in 1768, date of the first modern census. Little reliability can be claimed for the figures of 1591-94 and 1717; nevertheless, all the available indicators, together with the historical

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events of the period, point toward a stabilization or reduction of the Spanish population.

The eighteenth century marks the beginning of a cycle of population growth. A period of relative prosperity follows the Peace of Utrecht in 1713. The plague disappears, and, in spite of frequent outbreaks of yellow fever and smallpox, catastrophic mortality is effectively checked. From 1768 to 1900, the population doubles in size, rising from 9.309 million to 18.608; the growth appears to be steady, without sudden depressions and accelerations (as is shown in Table 1, where the country's demographic development is traced through the census enumerations).[3]

The 1717 enumeration of households, whose reliability is still severely in question, was followed by three censuses having nearly modern characteristics: those taken by Aranda in 1768-69, by Floridablanca in 1787, and by Larruga in 1797.[4]

Table 1

Growth of the Spanish Population, 1717-1910

Year	*Population (000)*	*Average Intercensal Rate of Increase (%)*
1717	7,500.0	—
1768	9,308.9	.42
1787	10,409.9	.59
1797	10,541.2	.13
1860	15,649.1	.63
1887	17,560.1	.43
1900	18,608.1	.45
1910	19,944.6	.70

Aranda based his census on the country's ecclesiastical subdivisions. As this method excludes from the count not only those villages belonging to the military orders but also those under the jurisdiction *nullius*, he probably understates the population. Nevertheless, for the first time, at least at the national level, the census unit was the individual instead of the household subject to tribute. Unlike Aranda's tabulation, the 1787 census (considered the most complete of the three) and the 1797 one (which, like Aranda's, probably understates the population) are based on a civilian subdivision of the country. The territorial data can, therefore, be compared with those of later enumerations.[5] Besides their use of the individual as the unit of enumeration, the three censuses of the second half of the eighteenth century have in common a sex-age distribution combined with marital status.

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A gap of sixty years separates Larruga's census from the first modern census, taken in nominative form in 1857. During this interval, many unreliable counts of the population were made, either officially or unofficially for administrative or fiscal reasons; none of these provides sex-age details.[6] The year 1857 marks the beginning of the modern statistical era; after 1857, censuses were taken in 1860, 1877, 1897, 1900, and every ten years thereafter. As far as vital statistics are concerned, official data on births, deaths, and marriages are unavailable until 1858. Between 1858 and 1870, data were collected by parishes and published with provincial detail. After the creation of a "civil register" in 1878, demographic events were systematically recorded by the civil authorities. The same official sources warn, however, that these statistics remain largely unreliable until the beginning of this century.[7]

The data in Table 1 show that the rate of Spain's population growth was steady from the eighteenth century to the beginning of the twentieth. Unlike England and Wales, the Scandinavian countries, and other Western populations, Spain did not experience a rise in the rate of increase and a major decrease in mortality as a result of a reduction in both "normal" and "catastrophic" mortality. As late as 1900, expectation of life at birth in Spain was below thirty-five years, a level surpassed by the Scandinavian countries one hundred and fifty years earlier.

The relatively stable rate of increase during the eighteenth and nineteenth centuries implies a parallelism in the trends of the birth and death rates. One must ask whether this stability was maintained through unchanged levels of fertility and mortality or through their equal change. Owing to a marked stabilization of living conditions—mainly the result of fewer shortages of foodstuffs—mortality must have declined during the first fifty or sixty years of the nineteenth century. Still, the recurrent epidemics of yellow fever, smallpox, and cholera probably reversed this trend fairly often.

Stable Techniques of Analysis and the Mortality of the Spanish Population

On the basis of the pioneering work of Alfred Lotka, a complex methodology has been developed for estimating vital rates from census age distributions. Lotka has mathematically demonstrated that every population experiencing the same mortality risks and

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fertility rates tends to assume an unvarying or "stable" age distribution, with constant birth, death, and growth rates. Because premodern populations often approximate conditions of stability, they can be treated as if they were stable.

Estimates of vital rates, based on stable techniques, are reached essentially by two successive stages. In the first stage, a model stable population is chosen. This selection can be made using two nonredundant parameters, such as rate of growth, level of mortality or fertility, or age distribution. In the second stage, the characteristics of the model—birth, death, growth rates, and so forth—can be assumed to represent those of the actual population. The work involved in selecting the model is greatly facilitated by the large number of pre-calculated stable populations corresponding to different levels of mortality and rates of increase.[8]

We may assume with a certain degree of confidence that the Spanish population approached stability during the eighteenth century, since the rate of growth was steady, catastrophic mortality was reduced, although by no means eliminated, and migration was negligible.

The census age distribution provides one of the two parameters needed in selecting the model; either the intercensal rate of growth or the level of mortality may be used for the other. Computation of the rate of growth is affected by the varying completeness of the eighteenth-century censuses. This type of distortion tends to be reduced, however, when the rate of growth is computed for a sufficiently wide interval. There is no direct and reliable information on the levels of mortality in Spain during the eighteenth century.

In an effort to estimate mortality, we have combined the 1768-97 rate of growth with the 1797 age distribution.[9] The 1768-97 period has been selected because the rate of increase appears to equal that of the 1717-68 period, and because the higher completeness of the 1787 census would have produced a rate of growth too high for 1768-87 and too low for 1787-97. The method outlined above leads to the following estimates: rate of growth (1768-97), 4.3 per thousand; birth rate, 42.3 per thousand; and expectation of life at birth, 26.8 years. The estimated level can be assumed to represent the mortality prevailing during the eighteenth century, once we have decided whether an expectation of life slightly below twenty-seven years is consistent with the evolution of mortality in Spain during the nineteenth and early-twentieth centuries; with the social, economic, and sanitary conditions of that time; and

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with the mortality levels then prevalent in Western populations.

The gains in expectation of life in Spain from the eighteenth century to the 1860's and '70's are very slight, reflecting only a moderate improvement in the country's sanitary conditions. That

Expectation of Life of the Spanish Population, 1797-1910

Date	Years
Prior to 1797*	26.8
1861–70**	29.1
1877–87**	28.9
1900**	34.8
1910**	40.5

* Our estimate.
** Inst. Geog. y Est.

mortality should have decreased during this period had to be expected. The plague disappeared—for causes still unknown, but probably related to the improvement of nutritional standards and the adoption of effective measures against its dissemination. Other epidemic diseases, such as yellow fever, smallpox, and cholera, appeared upon dissipation of the plague, but their consequences were less terrible than those of the plague. After the beginning of the nineteenth century, the deadly effects of smallpox were checked by the diffusion of Jenner's vaccination. Data collected by J. Nadal for the Cataluñian city of Palamos show a drastic decrease in infant mortality, beginning about 1820.[10] Living conditions and nutritional standards undergo a general improvement during the eighteenth and nineteenth centuries. Famines became more infrequent, and the improved system of communication and transportation allowed better internal redistribution of foodstuffs (particularly during the nineteenth century).[11] In 1834, Moreau de Jonnès, a Frenchman, could write in his statistical work on Spain: "Production of agriculture has doubled in quantity, exceeding all public needs, and famine does not come every third year as before to decimate the population of some province of the nation."[12]

Although reliable national life tables are lacking (with the exception of Sweden), levels of expectation of life below thirty years seem to have been quite common in Europe during the eighteenth century. According to R. Duvillard's table, the expectation of life of the French population before the Revolution was around twenty-nine years.[13] Judging from the incidence of extraordinary mortality, sanitary conditions were probably worse in Spain than in France. In Italy, expectation of life was around twenty-six years

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in Bologna (1811-12); twenty-eight in Verona (1761-66); twenty-nine in Liguria (1807-12) and Naples (1826-44); and thirty in Milan (1804-05).

A level of mortality corresponding to an expectation of life at birth of around twenty-seven years is, therefore, a likely estimate for eighteenth-century Spain. This estimate is consistent with both the evolution of social and sanitary conditions in the nineteenth and early-twentieth centuries and the mortality levels of other European countries, particularly France and Italy.

The Evolution of Fertility, 1768-1910

With the help of the stable techniques outlined in the preceding paragraphs[14] and using the official statistics of births after 1858, we have computed the following measures of fertility from 1768 to 1910: the birth rate; the marital fertility rate (MFR), or the number of legitimate births per 1,000 married females of fecund age (15-49 years); and a measure of marital fertility (I_g) standardized by age (ratio of the legitimate births of the actual population to the legitimate births that the married population would have produced had it experienced the maximum "empirical" fertility—that of the Hutterites). Because both MFR and I_g are independent of the changing marital status and age structure of the female population of reproductive age, they are more precise measures of fertility than the birth rate.

The fertility measures are supplemented by an index of the age structure of the married female population (the ratio of the married women aged sixteen to twenty-five to the married women forty to fifty). For 1858-62, 1900-1904, and 1908-1912, we have also indicated the proportion of illegitimate births over the total number of births; this proportion is unknown for earlier dates.

The estimates of the birth rates for 1768, 1787, and 1797 are based on the census age distribution, which is determined by the joint action of mortality and fertility over several decades previous to the census date. Even though the birth rate reflects a situation that antedates the census, we speak of the "1768," "1787," and "1797" birth rates for the sake of simplicity.

The data collected in Table 2 give an interesting picture of the development of Spanish fertility. The birth rate drops substantially between 1768 and 1860. This decline accelerates after 1860, and by 1910 the birth rate is less than three fourths the 1768 level.

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Table 2
Measures of Fertility, 1768–1910

Year	Birth rate	% Married females 16–50 over total population	% Illeg. births over total births	Married females 16-25/100 married females 40-50	MFR	I_g
1768	43.84	14.76	...	42.1	280.4[d]	.767
1787	43.16	14.71	...	48.4	277.0[d]	.745
1797	42.27	14.53	...	53.5	274.6[d]	.735
1860	39.50[a]	15.05[b]	5.6[c]	...	247.8	.659[b]
1887	37.03[a]	15.16	...	54.5	232.6	.616
1900	36.05[a]	14.82	4.4[c]	50.8	232.6	.622
1910	32.75[a]	14.36	4.8[c]	46.4	217.2	.582
		Index numbers, 1768 = 100				
1768	100.0	100.0	...	100.0	100.0	100.0
1787	98.4	99.7	...	115.0	98.3	97.1
1797	96.4	98.4	...	127.1	97.9	95.8
1860	90.1	102.0	...	...	88.4	85.9
1887	84.5	102.7	...	129.5	83.0	80.3
1900	82.2	100.4	...	120.7	83.0	81.1
1910	74.7	97.3	...	110.2	77.5	75.9

[a] Birth rates computed considering the average births for 1861–64, 1886–89, 1900–1903, 1909–1912
[b] Estimated value
[c] 1858–62, 1900–1904, and 1909–1912
[d] Values computed discounting an illegitimacy equal to the 1858–62 value

The decline between 1768 and 1887 becomes even more evident when we turn our attention to measures of marital fertility. By 1887, MFR and I_g are 17 to 20 per cent below the 1768 values (15 per cent for the birth rate). In the second half of the nineteenth century, therefore, both the population's marital structure and age composition were more favorable to a high number of births than they were during the eighteenth century. By 1910, marital fertility had fallen to slightly over three fourths the 1768 estimated levels.

By subdividing the country into four large regions, each having a certain social or geographic homogeneity, and analyzing regional data, we uncover certain interesting features.

The East experiences the largest decline, during both the 1768-1860 and the 1860-1910 periods; the over-all decline is inconspicuous in the Center and the North; in the South, fertility falls rapidly after 1860. The Catalan-speaking area, part of the former Crown of Aragón, shows a continuous decline that is certainly associated with the increasing diffusion of neo-Malthusian attitudes. H. Puig i

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Year	*Marital Fertility Rate (MFR)*				*Index Numbers, 1787 = 100*			
	East[a]	South[b]	Center[c]	North[d]	East	South	Center	North
1787	289	274	260	279	100	100	100	100
1860	243	261	242	247	86	95	93	89
1910	180	218	229	248	64	80	88	89

[a] The East: Old Crown of Aragón, Cataluña, Baleares; and Valencia
[b] The South: Andalusia and Murcia
[c] The Center: Castilla la Nueva, Extramadura, and León
[d] The North: Galicia, Castilla la Vieja, Vascongadas y Navarra, Asturias

Sais has indicated that the spread of certain contraceptive practices (primarily *coitus interruptus*) was the main factor in the fall of Catalan fertility.[15] Because Cataluña enjoyed a relatively high social level, was receptive to French influences, and entertained close cultural ties with other Catalan-speaking populations, it can be considered the ideal place for the start and diffusion of neo-Malthusianism in Spain.

The transition the Spanish population underwent between the eighteenth and nineteenth centuries would provide substantiation for a theory that marital fertility declined because of a wider use of birth control. The data on which we founded our analysis are, however, by no means perfect. It is well known that the eighteenth-century censuses are not blameless. We have assumed that age distribution was unaffected by census errors, but this assumption could prove to be wishful thinking. Moreover, the estimate of eighteenth-century mortality is little more than an elaborate guess, although one consistent with what is known about mortality in that period. Finally, we have assumed that the Spanish population, like other pre-modern populations, approached a situation of stability during this period. The precision and reliability of this approximation cannot be measured. Wars, epidemics, and extraordinary events of all sorts often prevented pre-modern populations from reaching stability, thereby distorting their age distribution and affecting the reliability of estimates based on stable techniques.

Nevertheless, little more can be done on the basis of the available data. We have aimed to test whether the fertility of the Spanish population remained stable from the end of the eighteenth century to the end of the nineteenth. Our evidence leads to the conclusion that fertility dropped, although prudential use of this finding must be recommended.

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Careful consideration has to be given to another aspect of the problem. The level of marital fertility remains substantially lower in Spain than it does in other Western populations.[16] Although

European Fertility Levels Circa 1870 (I_g)

Prussia	.90
Netherlands	.85
Norway	.76
Sweden	.71
Switzerland	.69
England and Wales	.68
Italy	.68
Ireland	.68
Spain (1860)	.66
Spain (1887)	.62

the diffusion of neo-Malthusianism was already under way among certain sectors of the other Western populations (upper classes, urban areas), Spanish fertility is well at the bottom of the list. Can biological factors alone explain the 30 per cent difference between Spain and Prussia, or must the action of possible volitional factors be taken into account when determining the causes of so large a gap?

If volitional factors must be considered, and if the decline from the end of the eighteenth century to the end of the nineteenth is real, we have created more problems than we have solved. Why did fertility decline in Spain, a deeply Catholic, strongly rural society with low social mobility and late, limited industrialization? Even when one accepts a "diffusion" or "adjustment" theory of fertility decline, the factors of the decline remain largely unexplained. According to this theory:

> Birth control, and especially contraception, need not be regarded as recent or new in human society. There may have been a "steady state" in which birth control was practised by part of the population, or it may have been practised with higher fertility targets. The decline in fertility is then regarded as an adjustment to a new set of forces, defining a new equilibrium level of modern or "controlled" fertility.[17]

The usual demographic explanations of fertility decline do not fit the Spanish experience. Although the secularization of life, the diffusion of education, and the process of industrialization and urbanization are probably important factors in the decline of fertility in many European countries and in Cataluña, their impact

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on the development of Spanish society was rather weak, at least during the first part of the nineteenth century.

Demographers should, perhaps, devote more attention to other, no less important, modifications in Spanish society. In 1787, one seventh to one eighth of all males about fifty years of age had never been married; by 1887, this proportion had fallen to one sixteenth. The reduced frequency of celibacy is, on the one hand, an effect of the decreased numerical importance of the clergy (6 per cent of the adult population by the mid-eighteenth century). On the other, it is a consequence of both the decreasing number of people claiming a noble origin(some 8 per cent of the total population in 1768) and the crisis of the institution of the *mayorazgo* (the practice of entailing land to the elder son). According to G. M. Jovellanos:

> The most notable evil (produced by *mayorazgos*)—if not the worst of all evils—falls on those same families for which the *mayorazgo* is created. Nothing is more repugnant than seeing the cadet sons of noble families, with no position and no career, condemned to poverty, celibacy, and idleness.[18]

Many class and caste preclusions to marriage were wiped out during the nineteenth century, in part because the war against France, "intermingling men of different classes, had destroyed many social divisions, many imaginary differences, many local rivalries, once insuperable obstacles to the foundation of new families."[19] It is possible that the disappearance of many preventive, or "Malthusian" checks to the birth rate (the elimination of the obstacles to marriage), gave way to the diffusion of a "neo-Malthusian" check, or the voluntary control of fertility within marriage.[20]

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13. Louis Henry, "The Population of France in the XVIII Century," *Population in History*, eds. Glass and Eversley (London, 1965), pp. 445-46. According to Henry, Duvillard's estimate, based on Halley's method, overestimates mortality. In fact, since the population of France increased during the eighteenth century, Duvillard's estimate of $^{0}e_{0}$ (28.8) has to be raised well over twenty-nine years.

14. For 1768, 1787, and 1797, the estimated level of mortality has been combined with the cumulative proportion of the census population (c_x) under seven, sixteen, twenty-five, forty, fifty, and sixty (only for 1797) years of age. Then for each of the six combinations of the mortality level and c_x, the appropriate stable population has been selected, and the corresponding birth rates recorded. Of the six estimates of the birth rate, the median has

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been selected as the "final" and "true" value for each sex; the two medians (one derived from the male and the other from the female age distribution) have been averaged to get the final estimate for the whole population. For 1860, 1887, 1900, 1910, the birth rate has been computed on the basis of the registered births adjusted for under-registration. (See the Appendix of my article, "Fertility and Nuptiality Changes in Spain from the Late XVIII to the Early XIX Century.")

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JEAN-NOËL BIRABEN

Certain Demographic Characteristics of the Plague Epidemic in France, 1720-22

THE EPIDEMIC that began in Marseilles in 1720 was the last important epidemic of the plague in Western Europe. Details about this epidemic can be gleaned from the abundant administrative reports that were made. On May 25, 1720, the *Grand St. Antoine* arrived at Marseilles. This vessel had left Saida (the Sidon of antiquity) in Syria on January 30, when the plague was raging in that country. It stopped at Tripoli and at Cyprus; it was refused entry at the port of Leghorn, however, because seven of its sailors had died en route, although their deaths were attributed to poor food and defective hygiene aboard ship. The health commissioners at the port of Marseilles took the precaution of impounding the ship's merchandise; they also quarantined the eight passengers and the crew for fifteen days in a lazaretto on an island in the middle of the port.

The passengers finally obtained permission to land on June 14; one went to Paris, two others to Holland. That same day, the sailors who had also been released came into contact with the people of Marseilles, who bought several contraband lengths of cloth from them. Shortly thereafter, the street porters who had carried the wares (cotton and wool material) as well as several of the sailors fell sick. They were taken to hospitals where they died one after another. The surviving crew members and all suspect merchandise were transferred to the Isle of Jarros; the captain of the ship, Chataud, was imprisoned in the Chateau d'If.[1] After an inventory, the confiscated merchandise was burned in September. The people in the city who had bought the contraband goods, however, fell sick and died rapidly: first a woman, then on June 28 a tailor and his family, and on July 1 two more women. By the following week, all the houses in the street contained invalids. These families were very

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poor, and it was not until July 9 that two doctors, Peyssonnel and his son, saw a case: They were called to the bedside of a sick thirteen-year-old boy, who died before their eyes within several hours. The malignity of the disease did not escape them, and they alerted the aldermen, who had the whole family hospitalized and ordered an investigation.

On July 14, the malady spread to another street, and a Dr. Sicard first spoke of plague. Dr. Bauzon, however, whom the aldermen had asked for an expert opinion, maintained that it was only a question of malignant fevers, possibly caused by intestinal worms. Public opinion became alarmed, but as no new cases occurred between July 15 and 21, it went from one extreme to the other. Dr. Sicard and the aldermen were accused of having frightened everyone for nothing.

On July 22, however, new cases were announced; fourteen people died on July 23. The aldermen began to think of extreme measures, but because many deaths were wrongly attributed to the plague, confusion continued for several more days. On July 29, the number of deaths increased suddenly; the municipality was overwhelmed by the urgency of the measures that had to be taken and of the help that had to be given. When, at last, a decree of the Parliament of Aix put the city of Marseilles under an interdict and the Chamber of Commerce *(Chambre des Vacations)* forbade all commerce between the city and the province, it was too late to stop the epidemic. Nearly ten thousand inhabitants had already fled.

At the same time, it was too early to close the city, for no stock of provisions existed, and food was lacking. After August 6, strikes broke out, protesting the rising prices and lack of bread. This agitation aggravated the pestilence, which rapidly became worse. By August 8, it was necessary to conscript beggars to bury the dead because the *corbeaux*, special bearers of plague-striken corpses, no longer sufficed for the task. On August 10, the malady had reached all quarters of the city. Municipal services were overwhelmed by August 11. The violence of the contagion left them completely disorganized. By the end of August, the plague had reached its maximum virulence. After August 15, it was no longer possible to bury all the dead. Employees counted on the day before fell sick daily. It was estimated that the enlisted drivers of the death carts worked, on an average, only two days; by the third, they fell in their turn. Of 217 convicts taken to bury the dead during the month

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of August, only twelve remained alive by the first of September. On this date, volunteers gathered together between twelve and thirteen hundred infants who had lost their mothers and tried to nourish them with soup and goat's milk. The babies died at a rate of thirty to forty a day.

During the entire month of September the plague raged and the dead accumulated. The new commandant, Langeron, arrived on September 4 and attempted to take the situation in hand. He succeeded, but not without difficulty. The Chevalier Roze offered his help and, with a group of forty volunteer soldiers, went to remove the mounds of cadavers from the Place de la Tourette, where no living person had dared penetrate for several days. All contracted the plague; the Chevalier Roze was lucky enough to be among the five who survived the disease.

Some statistics may give an idea of the violence of the epidemic. Of about ninety thousand inhabitants, ten thousand fled before the protective cordons were established; of the eighty thousand who remained, 39,334 died. All but three of the millers died. The surviving bakers fled on August 21 when they were forbidden to abandon their ovens, and it was necessary to enlist fifty volunteers immediately to replace them. In the butcher shops, the provisioners of livestock were miraculously immune, but only three meat-cutters remained alive after September 9. The personnel of the slaughterhouses all died, and the butchers were so decimated that it was necessary to replace them. Public services were practically nonexistent: All policemen, all captains except one, all lieutenants except two, all public servants (almost five hundred), the thirty guards recruited for the plague, and almost all of the three hundred and fifty men of the company of guards died. In the health services, of the twelve doctors who remained, six contracted the plague and four died; of thirty-five surgeons, thirty-two died. Various professions were affected differently, insofar as it is possible to estimate the number of members in each group before the epidemic and the subsequent number of deaths.

Master hatters:	50 deaths out of 100
Assistant hatters:	270 deaths out of 300
Carpenters:	84 deaths out of 134
Tailors:	78 deaths out of 138
Shoemakers:	110 deaths out of 200
Cobblers:	350 deaths out of 400
Masons:	350 deaths out of 500

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Out of a total of 784 galley convicts, 543 deaths were counted. Among the 696 put at the disposal of the city of Marseilles, 486 died, twenty-eight out of thirty-four in the stores selling wheat.

Thus, the epidemic continued, slowly becoming less violent. There were only several dozen cases between April 18, 1721, and August 19, 1721, when the last case was recorded. Two severe quarantines followed immediately: the first from August 20 to September 29, the second from October 1 to November 9. No new case was discovered; the city was freed at last. The work of restoring order was in full swing, when, on February 3, 1722, a relapse occurred which obliged immediate reinstatement of Draconian measures. This time only 260 persons were attacked, of which 194 died. The city lived in fear, however, and took a long time to recuperate from this last blow.

During this long trial, what was occurring outside Marseilles?

On August 1, 1720, a blockade of the city was instituted, although inefficiently at first, with only a few posts. On August 4, a decision was made to install protective cordons, but the necessary troops could not be assembled and put in place until August 20. From that time on, people could not cross the barriers; merchandise was allowed to pass only at four specified points, and infinite precaution was exercised. It was, however, much too late, as many of the escaped citizens had already brought the plague to numerous localities in Provence. The cordons were extended considerably and imposed on neighboring provinces, and precautions were extended to include all of France. These were imitated by Piedmont, Spain, Switzerland, and several other foreign countries.

Nevertheless, the plague was at Aix and at Apt on August 1, 1720. On August 15, it reached Aubagne. It spread to Toulon on August 20, where, after a long calm, it reappeared very violently on October 7. On October 12, it was at Martigues; on November 4, at Salon; on December 17, at Tarascon. It reached Marvéjols in Languedoc on July 12, 1721, entering Mende a little later, Avignon on August 17, and Alès on September 9.

It was at Mende at the end of September, 1722, and at Avignon on October 2. After having ravaged Provence, the Venaissin, and part of Languedoc for more than two years, it seemed that the plague had run its course. In Provence alone on August 31, 1722, there were 94,184 recorded cases; it is probable, in reality, that this figure exceeded one hundred thousand.

During the course of the epidemic, the government of Provence

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demanded a weekly report on the situation from all infected localities. A general report for all contaminated cities and places of Provence—giving for each the date of the plague's first appearance, the number of deaths, and the date of the last death or case—was drawn up which covered the period up to June 1, 1721.[2] A more complete register was compiled on October 1, when the epidemic had almost disappeared in Provence. In this province alone, the number of deaths due to plague reached 89,119 by October 1, 1721.[3]

We have tried to assemble the essential information into a picture of the plague in Provence[4]; comparable figures are not available for the Venaissin and Languedoc. The following table lists, in chronological order by first appearance, the results of the plague in the localities in Provence known to have been affected.

This picture contains important gaps and uncertainties.[5] The figures in parentheses are incomplete, as they were collected before the end of the epidemic. Few communities had, like Toulon, the leisure to take a census before the plague arrived; for most, there is only a rough evaluation taken from the head-tax rolls. The date of the first appearance of the plague corresponds sometimes to the date of the first known sickness, sometimes to that of the first death; occasionally an attack at first affected only one person or one family, but several months later carried off several thousand, as at Avignon. When a daily nominative list was kept, the number of deaths is sometimes fairly precise. Unfortunately, the people who kept these lists often perished, and their notes were burned. Thus, it was necessary to reconstitute the number of deaths, and only a very approximate reconstitution was possible. The figures for the population and for the number of deaths used in calculating the percentage of deaths in relation to the pre-epidemic population are full of errors.

With all its faults, however, this picture allows us to make a certain number of observations on demographic factors in an epidemic of plague. First of all, the localities do not seem to have been touched at random. They clearly tend to follow the important routes of communication of the period. The concentration of the population also seems to have been a decisive factor. Given the uncertainty of contemporary estimates of population figures and the absence of figures for those places that were unaffected, we have used the Census of 1764, which, on a relative scale, can be considered reliable as far back as 1720. If one looks at the *vigueries*, typical districts of southern France before the Revolution, of Aix, Apt, Brignolles, St. Maximin, Hyeres, Tarascon, Toulon, Marseilles,

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Demographic Characteristics of the Plague Epidemic in France

	Place	*Approximate Number of Inhabitants Before the Plague*	*Date of First Appearance of the Plague*	*Number of Deaths from the Plague*	*% of Deaths*
1	Marseilles and *terroir* (environs)	90,000	June 20, 1720	39,334	43.7
2	Cassis	3,500	July 21	216	6.9
3	Aix	24,000	August 1	7,534	31.4
4	Apt	6,000	August 1	271	4.5
5	Vitrolles	770	August 2	210	27.3
6	Ste Tulle	810	August 7	430	53.1
7	Aubagne	77,000	August 15	2,116	30.2
8	Mayrargne	850	August 15	382	44.9
9	Les Pennes Mirabeau	540	August 15	223	41.3
0	Gignac	470	August 15	42	8.9
1	Allauch	5,000	August 20	1,017	20.3
2	Toulon	26,276	August 20	13,283	50.6
3	Lançon	1,800	August 22	816	45.3
4	Roussillon	1,100	August 25	138	12.5
5	Septèmes	940	August 26	200	21.3
6	La Penne sur Huveaume	462	August 26	165	35.7
7	Le Puy Ste Réparade	900	August 29	43	4.8
8	St Canadet	125	August 29	34	27.2
9	St Zacharie	1,050	August 30	254	24.2
0	Gaubert	500	September 4	29	5.8
1	Rognac	370	September 18	148	40.0
2	St Cannat	1,500	September 22	342	22.8
3	Caseneuve	1,100	September 22	18	1.6
4	Corbières	400	September 22	131	32.8
5	Nans	500	September 27	125	25.0
6	Berre	2,000	September 28	1,080	54.0
7	St Martin de Castillon	1,500	September 29	326	21.7
8	Cucuron	3,500	October 1	832	23.8
9	Gardane	2,000	October 3	6	0.3
0	Auriol	3,200	October 3	1,319	41.2
1	Pourrières	1,200	October 4	1	0.2
2	Bandol	187	October 5	29	15.5
3	Perthuis	4,000	October 6	364	9.1
4	Pelissanne	2,200	October 6	223	10.1
5	Villars Brancas	300	October 9	12	4.0
6	Simiane	774	October 15	257	33.2
7	Le Canet	600	October 18	198	33.0
8	St Savourin	400	October 22	206	51.5
9	Carry	450	October 25	11	2.4
0	Martigues	6,000	November 1, 1720	2,200	36.7
1	St Rémy	3,000	November 1	1,005	33.5
2	Venelles	410	November 1	33	8.0
3	Salon	4,000	November 1	(894)	22.3

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	Place	Approximate Number of Inhabitants Before the Plague	Date of First Appearance of the Plague	Number of Deaths from the Plague	% of Deat
44	Rustrel	750	November 14	13	1.
45	Arles	22,000	November 26	9,400	42.
46	Vaugines	200	December 2	33	16.
47	Tarascon	10,000	December 17	210	2.
48	Mazaugues	441	December 17	168	38.
49	Géménos	1,100	December 20	54	4.
50	Orgon	1,700	December 29	105	6.
51	Ollioules	2,600	January 1, 1721	1,370	52.
52	La Seyne	5,000	January 4	2,006	40.
53	Maillanes	750	January 7	106	14.
54	Sues	58	January 18	7	12.
55	La Valette	1,660	February 20	1,068	64.
56	Ste Marguerite	276	March 20	22	8.
57	Forcalqueyret	230	May 27	174	75.
58	Le Revest	650	June 1	367	64.
59	La Garde (de Toulon)	544	June 3	300	55.
60	Trinquetaille les Arles	1,157	June 11	80	6.
61	Garéoult	1,200	June 13	168	14.
62	Ste Anastasie	500	June 14	310	62.
63	Cuers	3,000	June 27	?	?
64	Six Fours	2,226	June ?	40	1.
65	St Nazaire (Sanary)	1,200	July 1	51	4.
66	La Foux (le Puget)	213	July 3	98	46.
67	Roquevaire	2,500	July 9	46	1.
68	Frigoulet (abbaye)	60	July 12	19	31.
69	Néoules	450	July 17	313	69.
70	La Roquebrussane	997	August 14	201	20.
71	Graveson	1,100	August 15	11	1.
72	Noves	1,228	August 16	178	14.
73	Besse et Blanquefort	1,900	August 24	374	19.
74	Flassans	540	end of August	?	?
74	Lambesc	2,200	September 19	1	0.
75	Le Vernégues	440	?	?	?
76	La Cadière	2,480	?	?	?
77	Le Luc (près Toulon)	2,235	?	?	?
78	Solliès	4,895	?	?	?
79	Brignolles	4,831	?	?	?
	Comtat				
1	Avignon	23,041	September 23, 1720 to August 17, 1721	7,224	31.
2	Caumont/Durance	?	November 16, 1720	?	?
3	Bedarrides	?	August 15, 1721	?	?
4	Monteux	?	?	?	?

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inguedoc				
. Auxillac (Correjac)	109	November 23, 1720	58	53.2
! La Canourgue	1,633	December 8, 1720	945	57.9
3 Banassac	?	May 31, 1721	46	?
4 Marvejols	4,600	July 12	3,000	65.2
5 La Mouline	?	end of August	?	?
5 Bergonhon	?	end of August	?	?
7 Ste Colombe de Peyre	?	end of August	?	?
3 Montferrand	?	end of August	?	?
) Valladou	?	beginning of September	?	?
) St Léger de Peyre	?	beginning of September	?	?
l La Chaze	?	beginning of September	?	?
2 La Capelle (fontjulien)	?	September 2	?	?
3 St Georges (Serres)	?	September 2	?	?
4 Defret	?	September ?	?	?
5 Genolhac	?	beginning of September	?	?
5 Alès	10,000	September 9	336	3.4
7 Le Chambon (abbaye)	?	beginning of September	?	?
3 Lousgratous	?	mid-September	20	?
9 Altier (et Conzes)	?	mid-September	?	?
) La Rousse	?	mid-September	?	?
1 Chirac	?	?	?	?
2 Mende	?	?	?	?

and their *terres adjacentes* (environs)—all of which were heavily affected by the plague—one notes that the less populated localities were often less affected, and those that were are almost all along the main arteries of communication. Regardless of place, however, all cities of ten thousand or more inhabitants were affected.

Number of Inhabitants	*Total Number of Communities*	*Communities Affected by Plague*	*% of Communities Affected*
0–99	17	2	11.8
100–499	65	9	13.8
500–699	22	7	31.8
700–999	27	9	33.3
1,000–1,999	52	19	36.5
2,000–4,999	35	17	48.6
5,000–9,999	6	4	66.7
10,000 and more	4	4	100.0
Totals	228	71	

There seems to be no correlation between the size of the city and the violence of the epidemic. The distribution of localities according to the proportion of deaths in the population, however,

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seems significant. The following table lists the relationship for the seventy-eight localities with which we are acquainted.

Percentage of Deaths	*Number of Localities*
0.0–9.9	24
10.0–19.9	9
20.0–29.9	11
30.0–39.9	11
40.0–49.9	9
50.0–59.9	8
60.0–69.9	5
70.0–79.9	1
80.0–89.9	—

Such a distribution suggests the existence of two divisions: On the one hand, a large number of places had few deaths, sometimes only one; on the other, a large number were severely affected by the plague, losing between 20 and 50 per cent of their population. Between these two categories, there are only three places which lost between 15 and 20 per cent.[6]

Do epidemiological reasons explain this bimodal division? Are there two different explanations for the propagation of the plague? Because the plague follows communication routes, the role played by wild rodents would seem to have been less important than is true today in the Near and Middle East. Nevertheless, are urban rats always attacked? Are other factors at work? Does, for example, the pseudo-tuberculosis bacillus of Malassex and Vignal provide immunity against the plague bacillus? The question remains to be answered, but numerous factors in the plague epidemic of southern France in 1720-22 can be found which militate in favor of an hypothesis of Ricardo Jorge.[7] He argues that human fleas played an important part in the transmission of the plague in Western Europe from the Middle Ages until the present.

New studies are necessary on this subject; perhaps they will bring us more satisfactory results. The seasonal rhythm which rules the advances and retreats of the illness shows that climatic factors are also very important.[8]

REFERENCES

1. After a long trial his case was to be declared *nolo contendere* in a judgment of July 8, 1723. The registers of the comissioners of the Health Department do not contain a faithful transcription of his declaration.

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2. *Archives Nationales* G⁷1730.

3. *Archives Nationales* G⁷1733.

4. This information comes principally from the departmental archives of Bouches du Rhone, Series C.

5. This list gives a total of deaths from the plague in Provence of only 93,790. Documents of the period give this figure as 94,184, the difference coming from contemporary errors in addition and the gaps in our information.

6. The London epidemic of 1665 was much less violent than that of Marseilles; only about 15 per cent of the population died, and whole quarters remained untouched.

7. Ricardo Jorge, *Les anciennes epidémies de peste en Europe, comparées avec épidemies modernes* (Lisbon, 1932).

8. See also, Gaston Rambert, *Histoire du commerce de Marseille*, Vol. 4 (1660-1789; Paris, 1954); Paul Gaffarel and Marquis de Duranty, *La peste de 1720 à Marseille d'après des documents inédits* (Paris, 1911); Marc and Pierre Genton, *Contribution à l'étude historique de la peste dans la région Toulonnaise, au 18e siècle en particulier* (Paris, 1929). For the archive documents, see *Archives départementales des Bouches du Rhône*, Series C, Nos. 910, 916, 917, 918, 919, 920, 927, 944; *Archives Nationales, correspondance du Contrôle Général*, Series G⁷, Nos. for 1730, 1733, and 1736, and Series H, No. 1198.

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E. A. WRIGLEY

Mortality in Pre-Industrial England: The Example of Colyton, Devon, Over Three Centuries

In a recent article, T. McKeown and R. G. Record, analyzing the Registrar-General's mortality statistics for England and Wales in the nineteenth century, express forcefully the view that the substantial falls in mortality which occurred in the second half of the nineteenth century were due primarily to improved standards of living. Until the last years of the century neither public health measures nor medical therapy is accorded a major role in reducing death rates.[1] If marked falls in mortality rates occurred in the nineteenth century after the Industrial Revolution but before medical science or public health measures had contributed much to the struggle against early death, it is clearly possible that there were comparable fluctuations in mortality in earlier centuries. It would be as unwise to assume that mortality rates in pre-industrial times were invariably high as to make the same assumption about fertility rates.[2]

Unquestionably many factors besides living standards had a bearing on pre-industrial mortality rates. In the short term, weather, harvest conditions, and epidemic infections exerted a tremendous influence. In the longer term, general economic circumstances, the degree of urbanization, the density of settlement, changes in the virulence of particular diseases and their unpredictable appearances and disappearances, and genetic characteristics—all played a part. In various combinations, the interplay of these factors might produce very different levels of mortality.

There is clear, indeed dramatic, evidence of the crushing nature of the periodic crises which overwhelmed local communities in pre-industrial Western Europe when food prices shot up after bad harvests.[3] There is also much evidence of the terrible impact of the

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great epidemics of plague, smallpox, typhus, and influenza which might occur either independently or as part of a food crisis, attacking a weakened population with many people on the move from place to place and consequently liable to spread disease. There is, however, much less evidence about the levels of mortality which prevailed generally. Still less clear is the size of regional and local differences in mortality or the extent of change over time.

The comparative abundance of information about short-term crises and the relative lack of other studies are partly due to a technical problem in the analysis of the available sources. In England, for example, the parish registers are the only major source of information before the nineteenth century. They record by date and name the vital events of the parish—baptisms, burials, and marriages—and lend themselves readily to aggregative analysis. This permits analysis of the effects, say, of a harvest crisis because the month-by-month changes in the totals of burials, marriages, and baptisms can be correlated with data about the price of foodstuffs. Aggregative methods will not, however, yield age-specific mortality rates, and so permit the construction of life tables and the investigation of a wider range of questions, unless deaths are divided by age-group and an additional source is available giving totals of population at risk by age. Until 1813, when the provisions of Rose's Act of 1812 enjoined the recording of an age for each entry in the burial register, very few English parish registers record age at death, and censuses with detailed age divisions began only in 1841. Only in recent years has this problem been circumvented for earlier centuries by the development of the technique of family reconstitution using parish register entries.[4]

The purpose of this article is to present the results of a family reconstitution study of mortality in Colyton during the period 1538-1837 and to examine some of the problems of interpretation which accompany studies of this type. The analysis which follows is neither exhaustive nor definitive. The data on the family reconstitution forms (FRFs) have all been punched onto paper tape and will be more fully analyzed by computer shortly. When this has been done, and when parallel information for other parishes has become available, it will be possible to treat more fully some topics mentioned only summarily here or omitted altogether.[5]

The essence of the family reconstitution method is to assemble all the information about the vital events in a given family which can be gleaned from the register of a parish or group of parishes.[6]

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This is done in accordance with a set of rules designed to minimize the danger that wrong linkages will be made (for example, to try to ensure that the Elizabeth Green married on November 15, 1622 was the same Elizabeth Green baptized on April 6, 1593).[7]

Family Reconstitution and Mortality Studies

Each type of demographic calculation based on family reconstitution material has its own pitfalls. For example, a major problem in the study of infant and child mortality is to determine the period of time during which an individual is in observation. Clearly it is essential that the period be defined in such a way that it is independent of the event whose frequency of occurrence is to be measured. It would not do, for instance, to include the death of a four-year-old child in a count of mortality of children of that age if the only evidence of his presence in the parish were the recording of his burial, and other four-year-olds who did not die went uncounted for that reason. Failure to grasp the importance of this point in work of the reconstitution type can produce highly misleading results, as in a recent study of sixteenth-century York.[8]

In order to give a clearer picture of the problems of interpreting the Colyton data, it is perhaps useful first to describe briefly the type of information given in the burial register. The normal form of entry consists of the first name and surname of the deceased person, the date of his burial, and his relationship to the head of his family (except, of course, where the entry refers to the head himself). The description of the relationship to the head of the family is vital if links between baptism and burial entries are to be made with confidence. Where it is lacking (as it usually was in the Colyton register from 1709 to 1735), reconstitution, in general, and mortality studies, in particular, are always gravely handicapped and become impossible if the period of poor registration is prolonged. The relationship of a child to the head of the family continued to be recorded until he or she married. There are many entries in the form "son of" and "daughter of" where the son or daughter is fully adult, in his or her twenties, thirties, or even occasionally forties.[9]

In addition to this basic information, other details of the dead person are sometimes given—marital status, occupation, residence, cause of death, age, and so on. In the period 1670-79, for example, 561 burials were recorded, with details as follows: 90 "son of"; 84 "daughter of"; 87 "wife of"; 74 "widow"; 1 "bachelor"; 169 males

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and 56 females without indication of relationship or marital status. Of the 169 men, however, there were further details for 82: in 41 cases, place of residence; in 11, occupation; in 6, rank; 22 were distinguished as "the older" or "the younger," 1 was a bastard, and 1 a stranger. Of the 55 women, 17 were distinguished in a comparable fashion. At other periods, much more supplementary information was given. Between 1765 and 1779, for example, occupation was almost always recorded.

In the process of reconstitution, as many burial entries as possible are linked to a preceding baptism either directly, or indirectly by linking first to a marriage entry and then to a baptism. (In the case of married women, the middle link is, of course, essential since their surnames change at marriage.) Until the link has been made, age at death cannot be known unless it happens to be one of the supplementary items of information given in the register. Where an age at burial is recorded in the register, it is usually less accurate than an age calculated as the interval between baptism and burial, especially where death occurs at an advanced age.[10]

The necessity of conforming to independent criteria defining the period during which an individual is in observation tends to restrict mortality studies based on family reconstitution to infant and child deaths. The birth of later children in the family or the death of a parent provides in most cases evidence of the continued presence of the family in the parish up to a certain date. As long as the child at that date is under 10, or even under 15, there is a strong probability that he is still living with his parents. In this way, his presence in observation can be tested. The period 10-14 is more doubtful than earlier periods because of the increasing likelihood with growing years that a boy or girl will go out to service in another family, often in a different parish. This carries the attendant danger that death and burial may take place elsewhere.

Unmarried adults are at the other extreme. Their presence in the parish cannot be tested in any comparable fashion and, thus, no use can be made of information about their deaths. Married adults cause fewer problems, and estimates of adult mortality rates based on married people are possible, though the margins of error tend to be rather wide.

The Calculation of Mortality Rates

Rules for the calculation of mortality rates after families in a

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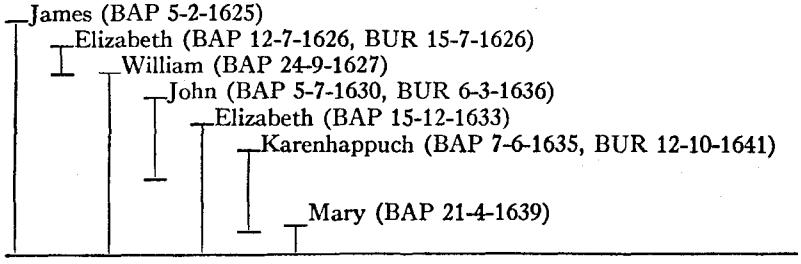
parish have been reconstituted were defined by E. Gautier and Louis Henry in their Crulai study.[11] Their exposition, with minor modifications enforced by the nature of the English parish registers, is followed here.

For the purpose of studying infant and child mortality, a family enters into observation from the date when the marriage is contracted or, in its absence, from the date of baptism of the earliest child recorded on the FRF. It passes from observation with the death of the later of the two parents to die where dates of burial for both of them are known. If the date of burial of only one parent is known, but it is also known that the other parent survived (as, for example, when a woman is described at burial as "wife of——"), this date marks the passage from observation of the family. If the date of burial of only one parent is known, but it is not clear whether the other parent died earlier or later, and also when the date of burial of neither parent is known, the family passes from observation on the date of baptism of the youngest child in the family. The limits of observation defined in this way apply to all the children in a family. Within these limits, any individual child is in observation from the day of his birth (or baptism) until his latest birthday which occurs within the period of observation defined. Figure 1 may help to illustrate the operation of these rules with an imaginary example.

Figure 1

Date of Entry Into Observation 3-9-1624
(William Bagshaw marries Elizabeth Hawkins)



Date of Passage from Observation 9-12-1641 (Elizabeth, wife of William Bagshaw buried—William's burial not recorded)

Children to the marriage:

James—in observation to fifteenth birthday
Elizabeth (I)—dies in observation in first year of life

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William—in observation to fourteenth birthday
John—dies in observation aged five
Elizabeth (II)—in observation to seventh birthday
Karenhappuch—in observation to sixth birthday
(death occurs outside observation and not counted in deaths of children aged 6)
Mary—in observation to second birthday

When details from all FRFs have been collated, life-table death rates (1,000 q_x) can be calculated. Thus, for example (in Table 1), 182 babies died in families which are in observation from their baptism to a point in time at least one year later. The total number of babies in observation for a full year from their birth is 1,679, including those who died. The infant mortality rate, therefore, is $\frac{182}{1,679} \times 1,000 = 108$ per 1,000. The number of children in observation for the second year of life is depleted by the infant deaths and by the passage from observation of 41 children between their first and second birthdays. This reduces the number in observation throughout their second year of life to 1,456 (1,295 children in observation until their second birthday or later, plus 161 children who died in observation aged 1 or more). Forty-nine children died at age 1, and this gives a mortality rate of 34 per 1,000 ($\frac{49}{1,456} \times 1,000$). Values are given for the life-table death rates age 0-1, 1-4, 5-9, and 10-14.[12]

The calculation of adult mortality rates is based on all married men and women whose age at marriage is known because a link has been made from the marriage entry to a preceding baptism entry. In analyzing the Colyton FRFs, only those marrying under 35 were included in this group. They married on an average at 25 years of age (see Table 10 below) and can be treated as entering observation at this age.[13] Their numbers were steadily depleted by death. The survivors at given ages can be calculated and related to the base population, thus permitting survival ratios and mortality rates to be derived. There is, however, a serious problem in using this material. It arises from the absence of information about the date of burial of a substantial fraction of the original group, and this introduces uncertainties into the calculation of all adult mortality rates. The problem can be met in two ways—by eliminating from the group as a whole subgroups peculiarly liable to be missing from the burial register; and by manipulating the data to produce high and low estimates of mortality between which the truth must lie. Both methods were used by Gautier and Henry in their study of

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TABLE 1: The Calculation of Life-Table Death Rates (1,000 q_x) for Infants and Children from Reconstitution Data: Colyton 1538–99

A *Age in years*	*B* *Deaths*	*C* *No. leaving observation*	*D* *Cumulative total of B*	*E* *Cumulative total of C*	*F* *D+E*	*G* *Death rate*
0	182		343	1,336	1,679	108
1	49	41	161	1,295	1,456	34
2	29	45	112	1,250	1,362	21
3	20	39	83	1,211	1,294	15
4	15	41	63	1,170	1,233	12
5	13	40	48	1,130	1,178	11
6	5	43	35	1,087	1,122	4
7	4	36	30	1,051	1,081	4
8	7	26	26	1,025	1,051	7
9	1	30	19	995	1,014	1
10	1	30	18	965	983	1
11	2	25	17	940	957	2
12	5	22	15	918	933	5
13	7	22	10	896	906	8
14	3	18	3	878	881	3
15		878				

Life-table death rates (per 1,000)

0–1	108
1–4	80
5–9	27
10–14	21

Crulai. After experimenting, for example, they eliminated from their female group all women whose husbands' dates of baptism were unknown because they proved especially likely to have moved from the parish and introduced an unacceptably large uncertainty into mortality calculations. Again, in order to obtain high and low estimates of mortality, ages at death which were unknown were distributed as if all had died at the date at which their existence was last attested in the register (as the baptism of a child, for example); and as if all had lived to age 60 and had then conformed to the age pattern of mortality observed in those whose age at death was known. When added to the known ages at death, the two alternative distributions can be made to yield high and low mortality estimates.

In analyzing the Colyton data, the following conventions were observed:

1. All those whose spouse's age is unknown were excluded

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wherever no further information of any sort was available on the FRF. There were very few men in this situation (only 8 in 165 in the period 1538-1624), but many more women (49 in 237 in the same period). The difference probably arises because of the prevalence of the habit of marrying in the bride's parish when the husband came from a different parish, though the couple would then usually settle in the husband's parish. In such cases the partners to the marriage are in observation only at the moment of their marriage and not thereafter. To include them increases the margin between the high and low estimates without compensating advantages.

2. High and low estimates were obtained by distributing the deaths of all whose age at death is unknown as if none had lived beyond 69, and deaths in the age divisions up to that age had been distributed as in the population whose age at death is known, and as if all had reached 60 and had then been reduced by death in the same way as those whose age at death is known.

As an example of the operations involved, the treatment of the FRFs of marriages formed 1538-1624 is described. There were 345 men and women who married in this period and whose FRFs survived the test of the first convention cited above. Their average age at marriage was 25.4 years. The age at death of 227 of them is known, and Table 2 shows the distribution of these ages.

TABLE 2: Distribution of Known Ages at Death of Married Men and Women of Marriages Formed 1538–1624 (sexes combined)

Under 40	*40–9*	*50–9*	*60–9*	*70–9*	*80 and Over*	*Total*
48	28	51	46	40	14	227

Table 3 shows what is known of the remaining 118. To obtain high and low estimates of the mortality experience of the group as a whole, two distributions of the deaths recorded in Table 3 must be made in conformity with the rules set out above in the second convention. Table 4 shows the results of this exercise. The horizontal totals line of Table 3 shows that 3 individuals did not reach 60 and 2 did not reach 80; they form the second line of Table 4A. The vertical totals line shows that none of the remaining 113 men

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TABLE 3: What Is Known of Those Whose Precise Age at Death Is Not Known

highest age at which known still to be living \ lowest age known not to have been reached	*Under 40*	*40–9*	*50–9*	*60–9*	*70–9*	*80 and Over*	*Total*
Under 40			2		2	88	92
40–9			1			19	20
50–9						4	4
60–9						2	2
70–9							
80 and Over							
Total			3		2	113	118

and women reached a minimum age higher than the 60-69 age group. All, therefore, can be distributed in the same proportions as the known deaths above 60, as shown on line three of Table 4A. The totals line of this part of the table then represents the distribution of deaths on the low mortality assumptions. In Section B of the

TABLE 4: The Redistribution of Deaths in Several Age-Groups According to Low and High Mortality Assumptions

A. Low mortality assumptions

	Under 40	*40–9*	*50–9*	*60–9*	*70–9*	*80 and Over*	*Total*
Known Ages at Death	48	28	51	46	40	14	227
			3		2		5
				52	45	16	113
Total	48	28	54	98	87	30	345

B. High mortality assumptions

	Under 40	*40–9*	*50–9*	*60–9*	*70–9*	*80 and Over*	*Total*
Known Ages at Death	48	28	51	46	40	14	227
	1		1				2
	25	14	26	25			90
			1				1
		5	7	7			19
			4				4
				2			2
Total	74	47	90	80	40	14	345

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table, a parallel exercise produces the distribution of deaths by age on high mortality assumptions. In this case, the vertical totals column is the main guide since it shows the number of men and women known to have reached certain ages (those who cannot have died earlier than the age groups in which they fall in the totals column). The top line of Table 3 shows that 2 individuals did not reach 60, and the distribution of known ages at death suggests that they should be allotted as shown on line two of Section B. The remaining 90 individuals on the top line of Table 3 may have died at any age up to 69, and their deaths are divided accordingly on the next line of Section B. Line four of the section consists of the one man known to have reached 40, but not 60 (he appears on the second line of Table 3). Immediately beneath him on line five are the other 19 individuals from the second line of Table 3. Finally, those on the third and fourth lines of the totals column in Table 3 are to be found on lines six and seven.

Table 5 shows the rate at which the population of 345 married

TABLE 5: Survivors to Certain Ages on Low and High Mortality Assumptions (actual numbers and related to a base population of 1,000)

Age	Low mortality	High mortality	Low	High	Medium
25	345	345	1,000	1,000	1,000
40	297	271	861	786	823
50	269	224	780	649	714
60	215	134	623	388	505
70	117	54	339	157	248
80	30	14	87	41	64

people is depleted on low and high mortality assumptions, and the same expressed in relation to a base population of 1,000, together with a medium estimate halfway between the two extremes. An average age at death can be calculated both for those whose age at death is known and for the distributions of deaths on low and high mortality assumptions. To calculate these figures, it is assumed that deaths in the six successive age groups occurred on average at 32, 45, 55, 65, 75, and 83 respectively.[14] The results are shown in Table 6.

The Mortality History of Colyton

Having described the methods by which a picture of the mor-

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TABLE 6: Average Age at Death

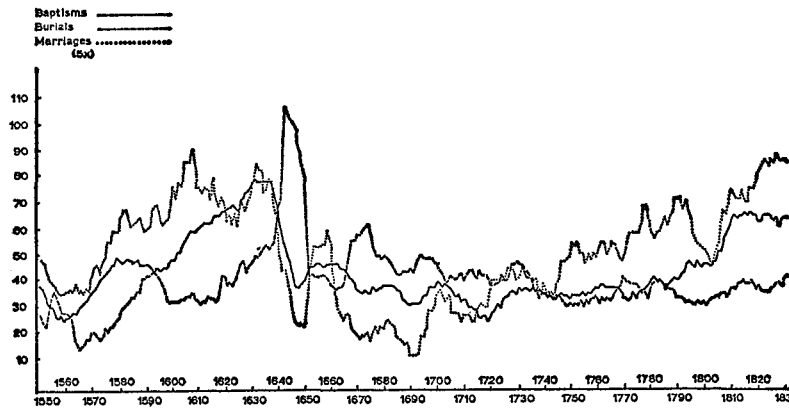
a.	Those whose age at death is known	56.2 years
b.	On low mortality assumption	61.3 years
c.	On high mortality assumption	54.5 years

Note: The two extremes vary from the middle point of the range by 3.4 years or 5.9 per cent.

tality of certain age groups can be built using suitable parish registers, we may consider what happened in Colyton between the sixteenth and nineteenth centuries. Figure 2 shows the fluctuations in baptisms, burials, and marriages in Colyton in the form of nine-year moving averages. It suggests four chief divisions of the three centuries as regards the relationship between births and deaths: An initial period of about a century during which population was apparently rising fast, baptisms were almost always more numerous than burials, and there was a marked inverse correlation between movements in births and deaths; a second period beginning in the 1640's and lasting until the 1730's, when the inverse relation-

Figure 2

Baptisms, Burials, and Marriages in Colyton
(nine-year moving averages)



ship continued, but with burials normally more numerous than baptisms and population apparently falling; a third period of half a century when baptisms and burials were very close to each other in number and without decisive trend; and a final period beginning about 1790 when baptisms, as in the first period, soared high above burials. If annual totals are consulted, one year in particular stands

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out in these three centuries, partially masked in Figure 2 by the moving average. Between the beginning of November, 1645, and the end of October, 1646, 392 names were written into the burial register, the great majority probably being victims of a last and violent outbreak of plague in the parish. The danger of depending upon Anglican parish registers for a complete coverage of vital events in an English parish at any time (but especially as the eighteenth century wore on) are well known, and it would be foolish to place too much credence in the absolute totals shown on the graph. Nevertheless, the major changes are shown up well enough.

The fertility changes which help to explain why the relationship between the crude totals of baptisms and burials changed so drastically have been described elsewhere.[15] Marital fertility was high until the 1630's, fell sharply for two or three generations thereafter, during which family limitation appears to have been widely practiced, recovered slowly during the middle years of the eighteenth century, and returned to Elizabethan levels by the early-nineteenth century. During the seventeenth and eighteenth centuries, there were also important changes in the average age at first marriage of women which tended to exaggerate the effect upon general fertility rates of the changes which occurred in marital fertility. Women married late in the second half of the seventeenth century, but early by the beginning of the nineteenth. Were there also changes in mortality between the sixteenth and nineteenth centuries? If so, were the periods of high fertility also periods of high mortality? Was the reverse the case? Or were the variations in fertility and mortality quite out of phase with each other.

Child Mortality

Table 7 gives the results of a series of calculations made by the method shown in Table 1. The figures in the table differ slightly from the few published previously[16] because the latter were the fruits of a preliminary calculation based only on those FRFs on which a date of marriage is recorded, whereas those in Table 7 are derived from all Colyton FRFs which meet the conditions described above, whether a date of marriage is known or not. The size of the populations at risk was of the same order of magnitude in each period. For example, Table 1 shows that the number in observation in the first year of life in 1538-99 was 1,679; the comparable figures for the four later periods are 2,077, 1,309, 985, and

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2,576, a total of 8,626—or 68 per cent of the total of baptisms in Colyton 1538-1837.

TABLE 7: Infant and Child Life-Table Death Rates ($1{,}000q_x$) in Colyton (sexes combined)

	1538–99	*1600–49*	*1650–99*	*1700–49*	*1750–1837*
0–1	108	81 (77)[a]	79	98	72
1–4	80	93 (85)	113	82	75
5–9	27	50 (32)	65	23	28
10–14	21	44 (24)	35	23	20

[a] Figures in parentheses for 1600–49 show the death rates which result after deducting all deaths which occurred during the plague period 1645–46.

The extremely low infant mortality rates are the most notable feature of Table 7 and call for further analysis, but they are most conveniently dealt with after examining child and adult mortality, and these are, therefore, discussed first.

Mortality in childhood varied considerably from period to period. In order to judge what the changes imply, it is instructive to compare them with the sets of specimen life-table death rates published by the Population Branch of the United Nations Department of Social Affairs. Each set is related to a particular expectation of life at birth. The rates are set out in Table 8, while Table 9 shows how many of an initial population of 1,000 at age 1 die before reaching 15 under the several U. N. specimen tables and the comparable figures for Colyton.

TABLE 8: Life-Table Death Rates ($1{,}000q_x$) for Various Expectations of Life at Birth Taken from U. N. Specimen Life Tables (sexes combined)[17]

	$^{o}e_{o}$ *27.5*	$^{o}e_{o}$ *30.0*	$^{o}e_{o}$ *32.5*	$^{o}e_{o}$ *35.0*	$^{o}e_{o}$ *37.5*	$^{o}e_{o}$ *40.0*	$^{o}e_{o}$ *42.5*	$^{o}e_{o}$ *45.0*	$^{o}e_{o}$ *47.5*
1–4	186	166	147	131	117	104	93	82	72
5–9	62	54	48	43	38	33	29	26	23
10–14	41	37	33	29	26	23	21	18	16

It is apparent from the two tables that death rates in the age groups 1-14 for children born in the period 1538-99, whether taken separately or combined, are closely similar to those of the U. N. table which relates to an expectation of life at birth of 45 years. In the next half century, 1600-49, even after excluding the heavy mortality of 1645-46, death rates appear to have risen somewhat and conform best to the U. N. figures for an expectation of life of

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TABLE 9: Numbers Dying Before Age Fifteen of a Population of 1,000 at Age One ($1{,}000_{14}q_1$) (sexes combined)

U. N. Specimen Tables		*Colyton*	
Expectation of Life	*Death Rate 1–14*	*Period*	*Death Rate 1–14*
27.5	267	1538–99	124
30.0	240	1600–49	176 (135)[a]
32.5	215	1650–99	200
35.0	192	1700–49	124
37.5	173	1750–1837	119
40.0	154		
42.5	138		
45.0	122		
47.5	108		

[a] The figure in parentheses for 1600–49 is the rate after deduction of all plague deaths 1645–6.

42.5 years. Death rates in the second half of the seventeenth century rose very sharply, particularly for children above the age of 5. Indeed, rates for the age groups 5-9 and 10-14 are high enough to suggest an expectation of life of less than 30, but the rate for the age group 1-4 is relatively much lower. The combined figure in Table 9 suggests an expectation of life of about 34 years, a drop of ten years or so from Elizabethan times.

In the next period 1700-49, the death rates suggest a return to the mortality conditions prevailing in the sixteenth century, but the rates for this period are minimal. There is a strong probability that the true rates would be considerably higher, for the burial register gives less detail in the early-eighteenth century than at other periods. The register at this time, and especially between 1709 and 1735, frequently mentions only the name of a deceased person without any other distinguishing characteristic. Experience in reconstitution shows that unless relationship to the head of the family is stated where appropriate, the difficulty of identification becomes very great. It is often impossible to decide between related people who bear the same name (whether, for example, the dead John Green is the uncle or the nephew, and so on). This tends to cut down the period of time a family is in observation because some dates of passage out of observation may prove impossible to determine. (It is symptomatic of this difficulty that fewer children were in observation in this period than at other times; see p. 552 above.) Even when a family technically conforms to the rules for

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being in observation, there remains a danger that a death may occur among the children in the family followed by an entry in the burial register which is too ambiguous to permit confident identification.

Before the middle of the eighteenth century, the register improved, and indeed in the 1760's and 1770's contains unusually full information by normal English parochial standards, though there were again short periods of poor detail at the turn of the century. From 1813, when registration of burials took place for the first time on printed sheets to conform to the requirements of Rose's Act, relationship was again not stated, but in compensation age at death was always given and ambiguities remain at a low level. By this period, the child death rates suggest an expectation of life at birth of about 45 years, as in the sixteenth century.

At all periods there was, of course, some under-registration of child deaths where these occurred during the temporary absence of the family in another parish. An arbitrary method of allowing for this is described below (see pp. 573-74).

Adult Mortality

The death rates of married people in Colyton fall well into the pattern of mortality suggested by the child death rates. Table 10 shows the main features of adult mortality revealed by the method of analysis described earlier, while Table 11 shows some com-

TABLE 10: Mortality of Married Men and Women Marrying Under Age Thirty-Five by Date of Marriage Cohorts (sexes combined)

	1538–1624	*1625–99*	*1700–74*
Number in group	345 (157m., 188f.)	285 (140m., 145f.)	239 (128m., 111f.)
Number whose age at death known	227 (112m., 115f.)	179 (86m., 93f.)	142 (76m., 66f.)
Average age at marriage (in years)	25.4	25.3	24.8
Average age at death			
a) where date of death known	56.2	52.2	57.7
b) on low mortality assumptions	61.3	58.6	63.3
c) on high mortality assumptions	54.5	50.8	54.9

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Survivors to Certain Ages (population at age 25–1,000) on Three Assumptions About Mortality

	1538–1624			*1625–99*			*1700–74*		
Age	*Low*	*High*	*Medium*	*Low*	*High*	*Medium*	*Low*	*High*	*Medium*
25	1,000	1,000	1,000	1,000	1,000	1,000	1,000	1,000	1,000
40	861	786	823	839	751	795	895	803	849
50	780	649	714	695	512	603	795	615	705
60	623	388	505	554	277	415	695	418	556
70	339	157	248	270	98	184	414	184	299
80	87	41	64	67	25	46	80	33	56

parable material derived from the U. N. specimen life-tables. Needless to say, both because there are high percentages of men and

TABLE 11: Survivors to Certain Ages (population at age 25–1,000) Derived from U. N. Life-Tables (sexes combined)[18]

Age	$^{o}e_{o}$ *30.0*	$^{o}e_{o}$ *32.5*	$^{o}e_{o}$ *35.0*	$^{o}e_{o}$ *37.5*	$^{o}e_{o}$ *40.0*	$^{o}e_{o}$ *42.5*	$^{o}e_{o}$ *45.0*
25	1,000	1,000	1,000	1,000	1,000	1,000	1,000
40	766	788	808	827	844	861	876
50	591	627	660	691	719	747	771
60	401	443	481	517	555	590	622
70	204	236	268	301	336	354	404
80	51	63	77	93	109	128	144

women whose date of death is not known and because of the varying sex composition of the three groups in Table 10, any interpretation of adult mortality in Colyton is subject to wide margins of error. The difference between the low and high mortality survivorship tables is very wide, so that the low mortality figures of the middle period when death rates were high show better survivorship rates than the high mortality figures for the other two periods. Nevertheless, for all its deficiencies, Table 10 does suggest strongly that the period 1625-99 suffered from substantially higher mortality than the earlier and later periods.

The tables of medium mortality (calculated simply by taking the midpoint between the two extremes) might appear to be the best general guide to the mortality changes which took place since it is most unlikely either that no person whose date of death is unknown died before 60 (low mortality hypothesis) or that all had died before reaching 70 (high mortality hypothesis). There is reason, however, for inclining to the view that the low mor-

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tality hypothesis is nearer to the truth, particularly for the later ages. Imagine, for example, that a quarter of all married couples emigrated ten years after marriage. During the period one such couple is in observation, if either the husband or wife dies, the death will be recorded in the register and reflected in the distribution of known ages at death. If no death occurs, however, all that will be known is an age at which the husband and wife passed from observation. There is, therefore, a bias in the distribution of known ages at death tending to overstate the number dying young. Relatively fewer young than old deaths will be missing from the table of known ages at death. Although the low mortality hypothesis is too extreme, it is probably nearer the truth than the high mortality hypothesis for the higher ages. It is always conceivable, of course, that this effect might be counterbalanced by an association between emigration and early death, but there is no evidence as yet on this point.

Bearing these considerations in mind, it is reasonable to suppose that in the period 1538-1624 the adult death rates conform best to the U. N. life-table relating to an expectation of life at birth of 40.0 years; that in the period 1625-99, they are closest to the table giving an expectation of life of 32.5 years; and in the period 1700-74 to the table for 42.5 years. Thus, the pattern of change in adult death rates, as with the rates for children, suggests a substantially lower expectation of life at birth in the second half of the seventeenth century than in Elizabethan times or in Georgian England. If the mortality data is broken up into short time periods, the number of individuals involved grows small, and firm conclusions necessarily become impossible. Nevertheless, it is interesting that if such subdivisions are attempted, they suggest that mortality rates tended to increase, though very slowly at first, from the late-sixteenth century until the third quarter of the seventeenth century, only to fall back toward their earlier level during the eighteenth, so that by its end they were no higher, and even possibly a little lower, than in Elizabeth's reign. Comparison of the Colyton death rates with the U. N. figures suggests that the whole cycle of change brought expectation of life down from a high figure in the range 40 to 45 years to a low in the range 30 to 35, before returning to its earlier level.

The periods used for the study of adult mortality differ from those used for child mortality. This is largely a result of the different techniques involved. Marriage cohorts beginning after 1775 include a growing number of men and women who were still living

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in 1837, the closing year of the Colyton study. Child mortality, on the other hand, can be examined right down to 1837 because passage from observation is precisely defined. Again, the lack of detail in the burial register in the early part of the eighteenth century makes it necessary to treat this period as a unit in the study of child mortality, but, even though the adult deaths are also harder to identify in this period, fewer problems are created. At worst, the percentage of deaths whose date is unknown is increased. Similarly, the differences between the techniques of analyzing the two bodies of data make it desirable to treat 1625-99, rather than 1650-99, as a block in the study of adult mortality because deaths from a marriage cohort are spread out over a much longer period than deaths under 15 from a birth cohort. The higher death rates of the late-seventeenth century affected cohorts marrying before 1650 as well as those marrying later. The plague year 1645-46 falls in this middle period and helps to account for the higher death rates, but its over-all impact was relatively slight in so long a period. Even if the marriages which took place before 1646 are excluded, the death rates are only slightly lowered.

The periodization problem, however, is a major difficulty in the analysis of reconstitution data; in a more extended study, it would be valuable to conduct an analysis in the form of a moving average —taking in and relegating ten-year or twenty-five-year cohorts from a moving block of data covering, say, seventy-five or one hundred years. This might also prove valuable in attempting to solve another teasing problem—the welding together of information about child and adult mortality into a single life-table. Children born in, say, 1580-89 married chiefly in the period 1600-19, and a significant fraction of the original birth cohort was still living in 1645 when the plague struck. Thus, to produce cohort life-tables, a time lag must be allowed for. For current life-tables, on the other hand, the information about death rates at all ages must come from the same span of time. Each kind of table has its special virtues. For example, a smallpox outbreak will leave largely untouched a cohort which in childhood has already been exposed to the disease, but will devastate younger cohorts. Current life-tables which reflect this state of affairs would produce a misleadingly high cumulative mortality up to the age at which cohorts had previously been exposed to the disease, misleading because no one birth cohort would be devastated twice by smallpox, although the method of calculation from current rates subjects the population to high mortality

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from the disease several times over. On the other hand, a war or a run of bad harvests may produce striking effects whatever the previous history of the cohorts at risk, and here current life-tables may convey a more telling picture of the impact of the blow than the alternative method.

Where the analysis periods are long, as is inevitable in a reconstitution study of a single parish, the distinction between the two types of measurement loses much of its point. For present purposes, therefore, and particularly given the wide margin of error in the adult death rates, it is sufficient to note the parallel movement over time of the child and adult rates and their close agreement in each major period when related to specimen life-tables.

Infant Mortality Reconsidered

Before commenting on the general import of the Colyton mortality data, infant mortality must be reconsidered. If the child mortality rates are taken to suggest that the appropriate specimen life-tables for the five periods are level 50 ($^{o}e_{o}$ 45.0), level 45 ($^{o}e_{o}$ 42.5), level 30 ($^{o}e_{o}$ 35.0), level 40 ($^{o}e_{o}$ 40.0), and level 50 ($^{o}e_{o}$ 45.0) respectively, then all the infant mortality rates calculated from the FRFs are apparently much too low, as Table 12 shows. In each

TABLE 12: Infant Mortality ($1{,}000q_0$) in Colyton and from U. N. Specimen Life-Tables (sexes combined)[19]

Colyton		*U. N. Specimen Life-Tables*	
Period	*Infant Mortality Rate*	$^{o}e_{o}$	*Infant Mortality Rate*
1538–99	108	45.0	159
1600–49	81	42.5	172
1650–99	79	35.0	214
1700–49	98	40.0	187
1750–1837	72	45.0	159

period, the table suggests that infant mortality rates should be between about one-and-a-half and two-and-a-half times the level produced by manipulating the parish register entries.

The risk that an infant death does not give rise to an entry in the burial register is greatest for very young babies before baptism. Once a child has been baptized, it is unlikely that its subsequent

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TABLE 13: Distribution of Infant Deaths Within the First Year of Life

	Days		*Weeks*	*Months*							
	0–	*1–6*	*1–4*	*0–*	*1–*	*2–*	*3–5*	*6–8*	*9–11*	*Total*
1538–99	23 (44)	26 (29)	41 (46)	90 (119)	22 (26)	20 (20)	26 (32)	12 (14)	12 (17)	182 (228)
1600–49	2 (36)	24 (29)	30 (35)	56 (100)	36 (41)	12 (19)	27 (35)	19 (22)	18 (24)	168 (241)
1650–99	3 (31)	15 (16)	21 (26)	39 (73)	15 (24)	6 (8)	19 (26)	12 (14)	12 (13)	103 (158)
1700–49	1 (18)	5 (7)	29 (32)	35 (57)	15 (17)	6 (9)	15 (20)	14 (16)	12 (12)	97 (131)
1750–1837	1 (54)	19 (22)	37 (41)	57 (117)	21 (29)	17 (22)	41 (46)	23 (28)	26 (27)	185 (269)

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death would go unregistered. It is, therefore, illuminating to study the distribution of ages at death within the first year of life. Table 13 shows the distribution of deaths within the first year of life by the apparent age of the dead child (in most cases date of burial minus date of baptism). The figures in parentheses are those which result from including those FRFs on which there were child burials without a preceding baptism entry for the child. In the overwhelming majority of cases, the child in question may be presumed to have died very young, and the timing of the other baptisms in the family shows an appropriate gap at the time of the burial. In principle, it is clearly improper in general to include such FRFs in an analysis based on strict criteria of presence in observation since the registration of the burial is the only evidence of existence. When a child died in the first days of life, however, it was usually only his death which prevented his baptism. In many cases, private baptism at home preceded the burial, but this was not always entered in the baptism register. Since almost all such burials could be given a "manufactured" date of birth on the same day as the burial, this greatly inflates the figure of infants dying on their first day of life. (A proportion of these deaths should have been allotted to the first week rather than the first day of life, but for simplicity all were given a "manufactured" date of birth on the same day as the burial where this was possible.) The figures for older babies rise too, but only because FRFs in this class include, of course, many other burials of young children which were preceded by a baptism in the normal way. If infant mortality rates are calculated using these FRFs as well as those in the main group, the rates for each rise are as follows (rates of Table 7 in parentheses): 119 (108), 92 (81), 95 (79), 113 (98) and 94 (72).

Absurdly low numbers of infant first-day deaths in the figures based on the main group of FRFs arise mainly because baptism did not usually take place on the same day as birth in Colyton, except in the earliest of the five periods. By the 1770's, when for the first time the baptism register contains dates of both birth and baptism, only half of all baptisms (50 per cent) took place within the first month of life (33 per cent in the third and fourth weeks). As many as a quarter of all baptisms were delayed more than three months after birth. Thus, at this time, the totals of deaths in the first month after baptism refer to the second month of life as much as the first. Some revision of the crude figures is clearly necessary. How should this be done?

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It is convenient to begin by considering the distribution of deaths under age one revealed by infant mortality studies of parishes where there is less reason to fear under-registration. Table 14 contains details of several such parishes. It shows that where there is little reason to doubt the completeness of registration, the percentage of all children dying under one year of age who died in the first month of life fell usually in the range of 45 to 60 per cent. Crulai had a higher figure, but Gautier and Henry stress the exceptionally high proportion of first-day deaths in the parish (30 per cent of all deaths under age one), and suggest that a substantial fraction of these deaths represents the burial of children born dead, but baptized by the midwife.[20]

P. Galliano goes so far as to eliminate all infants dying in these circumstances from his data, and this produces an over-all figure of 46 per cent for those dying under one month from a very large body of information. At Sainghin-en-Mélantois, both when all such children are included and when half have been eliminated, the proportion of children dying in the first month lies between 50 and 60 per cent. The figures for the parishes of Thézels-St.Sernin reveal similar, but lower percentages.

The York register is included in this table because for a time in the late-sixteenth century the register was kept with the most scrupulous care and included ages at death down to the nearest hour in the case of very young children. The deaths of 29 very young children were registered without any corresponding baptism entry, though usually with a note to the effect that baptism had been carried out at home, "being baptised at home by the midwyf." There seems more reason to fear that some children born dead were registered as if they had briefly lived than to expect under-registration in St. Michael le Belfrey at this period when 59 per cent of all deaths under age one were in the first month of life, a figure closely similar to those of Crulai and Sainghin-en-Mélantois. It is, incidentally, entirely possible that if the Colyton register had been kept in greater detail, the burial entries for which there was no corresponding baptisms would prove to have arisen in the same way as those in the register of St. Michael le Belfrey. (Much later, an eighteenth-century vicar of Linton in Cambridgeshire noted in his register: "It has been usual for many years past not to register the sickly children who are named at home, till they are brought to Church to be incorporated. Consequently, all that die and are never incorporated come into the List of Burials, but not of Baptisms.")

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TABLE 14: Distribution of Infant Deaths Within the First Year of Life[21]

		Days *0–*	*1–6*	*Weeks* *1–4*	*Months* *0–*	*1–*	*2–*	*3–5*	*6–11*	*Total*
Colyton 1538–99	a.	23	26	41	90 (49%)	22	20	26	24	182
	b.	44	29	46	119 (52%)	26	20	32	31	228
Colyton 1600–49	a.	2	24	30	56 (33%)	36	12	27	37	168
	b.	36	29	35	100 (41%)	41	19	35	46	241
St. Michael le Belfrey, York 1571–86		33	28	21	82 (59%)	9	8	20	21	140
Crulai 1688–1759		99		131	230 (69%)					331
	c.	50		131	181 (64%)					282
Sainghin-en-Mélantois 1740–89					163 (59%)	15	15	31	54	278
	c.				129 (53%)	15	15	31	54	244
Thézels -St.-Sernin 1747–92			62	34	96 (54%)	8	11	15	49	179
	d.		34	34	68 (45%)	8	11	15	49	151
19 parishes on the southern outskirts of Paris 1774–94	e.	264	308	333	905 (46%)	176	119	284	477	1,961

a. excluding all FRFs with "manufactured" dates of birth
b. including all FRFs with "manufactured" dates of birth
c. assuming that one half of all first-day deaths were born dead[22]
d. excluding the children baptized at home
e. Galliano eliminated all children who died after baptism at home by the midwife from his totals at risk and dying on the ground that probably a large proportion were born dead.[23]

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The modified figures for Colyton in 1538-99 (the "b" figures in Table 14) show a percentage of deaths in the first month of life little lower than that in the French parishes or at York, and indeed higher than in the large group of parishes just south of Paris studied by Galliano. In view of this, it would be difficult to find justification for adding more than, say, twenty-five deaths to the first month total in the modified Colyton data (to make the total 57 per cent of all first year deaths). This would raise infant mortality to only 131 per 1,000. Paradoxically, comparing the early Colyton data with the French material gives almost more warrant for supposing that there was under-registration of deaths in the second half of the first year of life than in the first half. In Sainghin-en-Mélantois, Thézels-St.-Sernin, and the nineteen parishes south of Paris, about a quarter of all first-year deaths fell in this period whereas in Colyton (and in St. Michael le Belfrey) the fraction was much smaller. Further study of the differences between French and English parishes in this respect might prove rewarding.

After 1600, the unmodified totals of deaths in the first days and weeks of life shown in Table 13 are clearly much too low in Colyton. Any method of inflating these totals is bound to be arbitrary since much of the information necessary for a satisfactory system of correction is lacking. As an illustration, however, of the effect of a plausible system of correction, the following procedure might be employed. Assume that deaths in the first month of life represent between 50 and 60 per cent of all deaths in the first year, and that the number of deaths in the period 1-11 months is understated because a proportion of those apparently dying under one month were, in fact, more than one month old, but their true age was concealed by the delay between birth and baptism (which would have the effect of reducing their apparent age). Assume further that during the seventeenth century one third of all deaths apparently under one month should be transferred to the 1-11 month group, and that thereafter one half should be transferred. A new range of infant mortality rates can then be calculated. As an example, in the period 1600-49, 56 children died under one month. One third of this number is added to the 1-11 month group to produce a new total for that group of 131, which is now taken to represent 40 to 50 per cent of all deaths under age one. The total of deaths now lies in the range 262 to 328, instead of 168; the infant mortality rate lies in the range 126 to 158 per 1,000.[24] Similar calculations may be made for other periods with the results shown in Table 15.

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TABLE 15: Revised Infant Mortality Rates ($1{,}000q_0$)

Period	*Infant Mortality Rate 120–140*
	(see text above for estimate of 131)
1538–99	120–140
1600–49	126–158
1650–99	118–147
1700–49	162–203
1750–1837	122–153

The revised figures are higher than those given previously, but they are still below, and in most cases substantially below, those to be expected from the U. N. life-tables. Are they necessarily suspect for this reason?

T. H. Hollingsworth suggests elsewhere in this issue (pp. 424-25) that infant mortality rates in pre-industrial Europe below the range of 200 to 250 per 1,000 are unlikely to be accurate. Comparison of his data about the British aristocracy with the Colyton data suggests that it may be premature to generalize in this way. Table 16 shows the infant mortality rates of the British aristocracy and also their life-table death rates age 1-14 and some comparable Colyton material.

Two points about the mortality of the British aristocracy revealed in Table 16 should be noted. First, infant mortality only once exceeded 200 per 1,000 between the sixteenth and nineteenth centuries. This happened during the bad years of the third quarter of the seventeenth century. It may seem reasonable to expect a lower infant mortality among the aristocracy than in a parish community, but until more is known of the childbed conditions, the exposure to infection, and the suckling habits of mothers and babies in the two groups, the issue must remain uncertain. Secondly, the remarkable lack of parallelism in the behavior of the death rates age 0-1 and 1-14 deserves stress. Although, in the main, when one series is rising, the other tends to move in sympathy and vice versa, there were notable differences in the speed and extent of the changes involved. In the late-sixteenth century, the rate 1-14 is little more than half the infant mortality rate, whereas by the mid-eighteenth century it was considerably the higher of the two. Toward the end of the eighteenth century, infant mortality was little more than half what it had been in the late-sixteenth, yet over the same period, mortality 1-14 was a third higher. Each rate had first risen and then fallen in the interim, but in widely different

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TABLE 16: Life-Table Death Rates ($1{,}000q_0$ and $1{,}000_{14}q_1$: sexes combined)[25]

	British Aristocracy			*Colyton*	
	0–1	*1–14*		*0–1*	*1–14*
1550–74	182	99	1538–99	120–140	124
1575–99	189	93	1600–49	126–158	176
1600–24	189	128	1650–99	118–147	200
1625–49	199	162	1700–49	162–203	124
1650–74	210	200	1750–		
1675–99	196	163	1837	122–153	119
1700–24	169	181			
1725–49	166	199			
1750–74	102	143			
1775–99	85	123			
1800–24	82	102			

proportions. There is in this table no warrant for the view that the two rates are closely related to each other. Therefore, it does not follow that where infant mortality rates are unknown or thought to be too low, they can be read off from a knowledge of death rates later in childhood. If we judge by the relationship between the two rates shown by the aristocracy data in the late-sixteenth century, the Colyton figures in Table 16 will seem improbable, but if we judge by their relationship in the early-eighteenth century, this will not be so.

There are other reasons for caution in assuming that infant mortality rates in pre-industrial communities were always high. Indeed, perhaps pre-industrial European communities will prove frequently to have differed in this respect from the developing countries today on whose data the U. N. life-tables were based. It is notable, for example, that the infant mortality rate in Crulai is below what would be expected on the basis of mortality 1-14 and comparison with U. N. life-tables. The Crulai infant mortality rate corrected for possible under-registration was put at 210 to 230 per 1,000 by Gautier and Henry.[26] This estimate includes, however, the large and doubtful group of babies baptized at home and suspected often to have been born dead. If half the children apparently dying on the first day of life fall into this class, the probable range becomes 180 to 200 per 1,000, whereas the death rate 1-14 ($1{,}000_{14q1}$) at 212 suggests an expectation of life at birth of 32.5 years (see Table 9) and an infant mortality of 229 per 1,000.

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The over-all conclusion of Gautier and Henry, that expectation of life in Crulai was about 30 years,[27] suggests an even higher infant mortality figure of 245 per 1,000.

This is not to say, of course, that infant mortality was never high in pre-industrial England. At St. Michael le Belfrey, for example, in the period 1571-86 when registration was so thorough, there were 566 baptisms, to which must be added the 29 children buried, but not baptized. This produces a total of 595 live births. In the same period, there were 140 burials of infants under age one, or an infant mortality rate of 235 per 1,000. St. Michael le Belfrey was a comparatively wealthy urban parish. In the poorer urban parishes, particularly in London, infant mortality may well have been much higher.[28] It was high in some rural parishes too. In the tiny marsh parish of Wrangle in Lincolnshire, for example, infant mortality in the period 1597-1642 was 240 per 1,000.[29]

Mortality as a Whole in Colyton

There are substantial uncertainties about the level of both infant and adult mortality rates in Colyton which impose great caution in advancing suggestions about expectation of life in different periods. At an earlier point in the argument, I suggested that the child and adult mortality data are consonant with a fall from an expectation of life at birth between 40 and 45 years in the second half of the sixteenth century to a low point of 30 to 35 years a century later, followed by a rise to its earlier level by the end of the eighteenth century. This question can now be re-examined in the light of the discussion of infant mortality. Table 15 shows that although the range between the high and low estimates of infant mortality is quite wide, there was no apparent tendency for rates to rise in the seventeenth century and that the peak was probably reached early in the eighteenth. If these estimates are to be relied upon, it is evident that infant mortality rates and those of later age groups were not closely in step with each other. Because infant mortality was low in the seventeenth century, the addition of infant mortality data to other mortality information tends to damp out the differences in expectation of life between this period and earlier or later times.

Combining the infant, child, and adult mortality data, it is possible to calculate high and low estimates of expectation of life at birth. For this purpose, the period as a whole was divided into

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three broad subperiods—1538-1624, 1625-99, and 1700-74—and the following procedure was adopted:

Infant mortality

1538-1624 High and low estimates were made by weighting the high and low estimates for 1538-99 and 1600-49 in the proportion 2:1 to derive a composite figure for the period as a whole. Thus, the high estimate of infant mortality in 1538-99 is 140 per 1,000, and in 1600-49 it is 158. The composite estimate is $\frac{(2 \times 140) + (1 \times 158)}{3} = 146$ per 1,000.

1625-99: The high and low estimates for 1600-49 and 1650-99 were weighted in the proportion 1:2.

1700-74: The high and low estimates for 1700-49 and 1750-1837 were weighted in the proportion 2:1.

Mortality 1-14

1538-1624 Low estimate: the average of rates for 1538-99 and 1600-49 (excluding the plague year) taken for each age group. High estimate: the low estimates plus 10 per cent (to cover possible under-registration due, for example, to the death of children when family temporarily absent from parish).

1625-99 Low estimate: the rates for 1650-99. High estimate: the same plus 10 per cent.

1700-74 Low estimate: the rates of 1700-49 plus 10 per cent (a notional allowance to offset the under-registration due to absence of detail in burial register). High estimate: the same plus a further 10 per cent.

Mortality 15-24

1538-1624 Low estimates: U. N. life-table death rates level 50 ($^0e_0$ 45.0 years). High estimate: level 40 ($^0e_0$ 40.0).

1625-99 Low estimate: level 30 ($^0e_0$ 35.0). High estimate: level 25 ($^0e_0$ 32.5).

1700-74 Low estimate: level 50 ($^0e_0$ 45.0). High estimate: level 40 ($^0e_0$ 40.0).

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Mortality 25–

1538-1624 Low estimate: U. N. life-table death rates level 45 ($^0e_0$ 42.5). High estimate: level 30 ($^0e_0$ 35.0).

1625-99 Low estimate: level 30 ($^0e_0$ 35.0). High estimate level 20 ($^0e_0$ 30.0).

1700-74 Low estimate: level 50 ($^0e_0$ 45.0). High estimate: level 30 ($^0e_0$ 35.0).

If the high and low mortality rates for each age range are combined in a single expression, high and low estimates of expectation of life at birth can be made.[30] They are shown in Table 17. There is no special magic about these estimates. All that can be said is that

TABLE 17: High and Low Estimates of Expectation of Life at Birth in Colyton (in years)

Period	*High Mortality*	*Low Mortality*	*Midpoint*
1538–1624	40.6	45.8	43.2
1625–99	34.9	38.9	36.9
1700–74	38.4	45.1	41.8

it is likely that the true figures of expectation of life would lie between the high and low mortality estimates. The figures cannot be said to relate in any precise sense to the periods listed for reasons which have already been discussed. They, and the material in earlier tables, show only that death rates were surprisingly low in late-Tudor Colyton; that by the Restoration period, they had risen sharply to levels broadly comparable to those in eighteenth-century Crulai (except for infant mortality which explains the higher expectation of life at birth in Colyton); and that, as the eighteenth century wore on, death rates fell back towards their Tudor levels.

Many aspects of infant, child, and adult mortality in Colyton would repay more extensive analysis than has been attempted here. No division of the material by sex has been made, for example, though there is much of interest to investigate (female rates were apparently higher than male in the adult married population in the sixteenth and seventeenth centuries). The problem of correcting the infant mortality rates has not been exhausted. The whole question of the interplay of fertility and mortality, the changes in reproduction rates over time, deserves full analysis. These are, however, deliberate omissions.[31] I have attempted to do no more than to show

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that suitable sources and appropriate techniques exist to enable mortality in many pre-industrial European communities to be studied in greater detail than once seemed possible. I have pursued the analysis of mortality in one parish far enough to leave little doubt that expectation of life at birth might change considerably in the course of two and a half centuries.

What can be learned from the study of one parish in Devon is necessarily limited. Colyton may have been typical of much of England, but equally it may have been exceptional. Its mortality history, like its fertility history, shows only that big changes *may* be found in the past, not that big changes were common or sufficiently widespread and uniform to affect regional or national aggregates. Historical demography is in its heroic age when exciting, and sometimes puzzling, discoveries are to be expected. No mass of carefully worked material as yet exists against which new discoveries can be measured. The significance of what happened in Colyton cannot be assessed without a mass of comparable data. For some time, therefore, there will be increasing marginal returns to research effort in historical demography. Each new parish register or listing of inhabitants analyzed will help to narrow the uncertainties raised by earlier studies as well as raising problems and opportunities of its own.

When more is known, for example, about the mortality of different occupational and social groups in pre-industrial populations, the degree to which poverty increased mortality will be easier to assess. (Some parish registers state the occupation of the head of the family when baptism and burial entries are made, as in the register of Banbury, Oxfordshire, after 1707.) Similarly, when it becomes possible to compare the demographic history of parishes with different types of economy, or those parishes on major traffic arteries with those comfortably remote from the danger of casual infection, or those parishes on marshland with the surrounding higher land, the interplay of demographic, economic, social, and other variables will grow less mysterious than at present when our ignorance extends so widely. Most important of all, a fuller knowledge of changes in fertility and mortality in parishes all over England at the time of the Industrial Revolution may serve to make clearer many functional relationships at present much shrouded in obscurity.

The manipulation of data to provide estimates of fertility and

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mortality in the past is only a first step in the general study of the historical demography of a community. The knowledge, for example, that mortality in Colyton varied considerably in the course of three centuries does not in itself serve either to substantiate or disprove for Colyton in earlier years the general hypothesis of McKeown and Record for the nineteenth century—that changes in living standards played the major part in changing mortality rates. There is already reason to expect that the Colyton mortality pattern may prove to be repeated in many parishes since simple aggregative studies of baptism, burial, and marriage totals so often show either burial surpluses or much reduced baptism surpluses in the later seventeenth century.[32] In England as a whole, however, there seems no reason to suppose that living standards reached a nadir in the late-seventeenth century.[33] Indeed, there might seem more reason to find support in the Colyton data for J. D. Durand's recent thesis—that the exchange of infectious diseases between the continents following the triumphs of European navigation in the sixteenth century increased death rates all over the world in the seventeenth.[34] Only the collation of the results of detailed studies extending into many aspects of parish life can give support to some hypotheses and enable others to be put out of court.

The strictly demographic data about communities in the past are significant in their own right, but they are of prime historical importance because a community's demography was sensitive to many influences in its economic, social, and physical environment. Pierre Goubert's study of the Beauvaisis provides a classic demonstration of this point.[35] Moreover, the demographic changes of the past were not simply passive reflections of change elsewhere in the life of the community. They also helped to determine in their turn the economic fortunes and social circumstances of the times.[36] Only when the demographic changes which occurred in parishes like Colyton can be related with confidence to other changes in parish life will the full value of good fertility and mortality series be realized. Potentially, however, they provide a nexus into which other types of information can be tied so that the relationships between them can be better understood. The fact that the changes can be quantified facilitates the testing of hypotheses about the relationships involved. Few aspects of the pre-industrial past can be quantified with such precision as fertility and mortality trends where suitable records exist,[37] and, by a fortunate coincidence, what it is possible to do, it is also important to do.

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What remains to be done bulks much larger than what has been achieved so far in historical demography. Where, as in England from the mid-sixteenth century onwards, the source material for detailed demographic studies is comparatively rich, there are great opportunities for improving knowledge of economic and social change and function. The chance to know more about the demographic attributes of pre-industrial communities is not one to be missed.

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2. For evidence of fluctuations in fertility, see E. A. Wrigley, "Family Limitation in Pre-Industrial England," *Economic History Review*, Second Series, Vol. 19, No. 1 (1966), pp. 82–109.

3. See, for example, Pierre Goubert, *Beauvais et le Beauvaisis de 1600 à 1730* (2 vols; Paris, 1960), esp. Part 1, Ch. 3.

4. See M. Fleury and L. Henry, *Nouveau manuel de dépouillement et d'exploitation de l'état civil ancien* (Paris, 1965); and E. A. Wrigley (ed.), *An Introduction to English Historical Demography* (London, 1966), Ch. 4.

5. Family reconstitution studies on the following parish registers are well advanced: Hartland, Devon; Banbury, Oxfordshire; Easingwold, Yorkshire; Aldenham, Hertfordshire; and a group of contiguous parishes in Bedfordshire (Campton-with-Shefford, Southill, and Clophill).

6. Where possible, it is often best to work on a group of neighboring parishes since this enables many individuals migrating short distances to be kept in observation. The frequency of short-distance migration is the greatest single factor tending to reduce the proportion of a parish's population whose demographic characteristics can be studied.

7. For details of the rules to be followed, see Wrigley (ed.), *An Introduction to English Historical Demography*, Ch. 4; also Wrigley, "Some Problems of Family Reconstitution Using English Parish Register Material," cyclostyled paper for Section 7, Third International Economic History Conference, Munich, 1965.

8. See U. M. Cowgill, "Life and Death in the Sixteenth Century in the City of York," *Population Studies*, Vol. 21, No. 1 (1967), pp. 53-62. In this study, the ages at death of 34 per cent of the children born in six York parishes between 1538 and 1601 were identified, and implicitly the assumption was made that the ages at death which could not be discovered were distributed in the same way as those which had been established.

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There is, however, a much better chance of linking a baptism entry to an early death than to later deaths and, in consequence, mortality was greatly overestimated. (Almost half the birth population is shown as dying in the first year of life. See Table 2, p. 56.)

9. Mary, daughter of Barnard Vickary, is an extreme example of this. She was described in this way when buried in 1697, aged 46 (her father died in the following year, aged 79).

10. In the early-nineteenth century, when baptism was frequently delayed for several weeks or months after birth, an age at death given in the register is frequently more accurate for young children than an age calculated as the interval between baptism and burial.

11. E. Gautier and L. Henry, *La population de Crulai* (Paris, 1958), Ch. 8; see also L. Henry, *Manuel de démographie historique* (Paris, 1967), Ch. 6.

12. See Henry, *Manuel de démographie historique*, pp. 125-28, for further details of methods of calculation.

13. See Gautier and Henry, *La population de Crulai*, p. 179, for discussion of this question.

14. The average age of a sample of fifty married persons dying under age 40 in Colyton was 32.2 years. In all, thirty men and women died at age 80 and above at an average age of 83.2 years (the highest age at death was 91 years). For other age groups, it was assumed that deaths were spread evenly and that the midpoint age could be taken.

15. Wrigley, "Family Limitation in Pre-Industrial England."

16. *Ibid.*, p. 101.

17. United Nations, Department of Social Affairs, Population Branch, Report No. 25, *Methods for Population Projections by Sex and Age* (New York, 1956), p. 74. The death rates for the sexes combined are a simple average of the male and female rates.

18. United Nations, *Methods for Population Projections by Sex and Age*, p. 76. Totals of survivors for the sexes combined were taken as a simple average of the male and female figures.

19. United Nations, *Methods for Population Projections by Sex and Age*, p. 74. The infant mortality rates for the sexes combined are a simple average of the male and female rates.

20. Gautier and Henry, *La population de Crulai*, p. 170.

21. For Crulai, see Gautier and Henry, *La population de Crulai*, pp. 169-70; for Sainghin-en-Mélantois, see Henry, *Manuel de démographie historique*, p. 135; for Thézels-St.-Sernin, see P. Valmary, *Familles paysannes au XVIIIe siècle en Bas-Quercy* (Paris, 1965), p. 147; for the nineteen parishes, see P. Galliano, "La mortalité infantile (indigènes et nourrisons)

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dans la banlieue sud de Paris à la fin du XVIIIe siècle (1774–1794)," *Annales de démographie historique 1966* (Paris, 1967), p. 159.

22. See Henry's remarks on Sainghin-en-Mélantois, *Manuel de démographie historique*, p. 137.

23. Galliano, *Annales de démographie historique 1966*, p. 143.

24. This range of infant mortality rates and those for later periods are approximate calculations made by inflating the rates given in Table 12 in the ratio of new totals of children dying under one to the original totals. Thus, for 1600–49, the lower end of the range is calculated as $81 \times \frac{262}{168} = 128$; and so on. In a stricter calculation, the larger number of children in observation and at risk which results from the inflation of the number of deaths should be taken into account, and slightly lower infant mortality rates would result.

25. The rates for the British aristocracy were calculated from data in T. H. Hollingsworth, *The Demography of the British Peerage*, Supplement to *Population Studies*, Vol. 18, No. 2 (1964), Table 52, p. 67. Totals for the sexes combined were taken as a simple average of the male and female figures.

26. Gautier and Henry, *La population de Crulai*, p. 168.

27. *Ibid.*, p. 192.

28. Urban poverty, very high population densities, and high infant mortality seem to have been closely associated in pre-industrial Europe.

29. Wrigley (ed.), *An Introduction to English Historical Demography*, p. 157.

30. The approximations used in calculating expectation of life at birth are those described in United Nations, *Methods for Population Projections by Sex and Age*, pp. 23–24.

31. I hope to take some of these questions further in a comparative study of the results of family reconstitution in several parishes.

32. The Cambridge Group for the History of Population and Social Structure now has aggregative returns for four hundred English parishes. When the material has been fully analyzed, the degree to which this pattern is apparent in different types of parish and in different regions will be much easier to specify.

33. In a recent and authoritative review of English economic history in the seventeenth and eighteenth centuries, for example, Wilson remarks: "After a half century during which depression and maladjustment had been a chronic feature of English economic life, the Restoration was the occasion for economic as well as political renewal." C. H. Wilson, *England's Apprenticeship* (London, 1965), p. 160. There seems good reason to regard this as a just summary of the contrast between the two halves of the century.

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34. J. D. Durand, "The Modern Expansion of World Population," *Proceedings of the American Philosophical Society*, Vol. 111, No. 3 (1967), p. 142.

35. Goubert, *Beauvais et le Beauvaisis.*

36. I have recently tried to analyze one set of relationships which illustrate this point. E. A. Wrigley, "A Simple Model of London's Importance in Changing English Society and Economy 1650-1750," *Past and Present*, No. 37 (1967), pp. 44-70.

37. This thesis is discussed at greater length in a forthcoming book, *Population, Economy and History.*

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D. V. GLASS

Notes on the Demography of London at the End of the Seventeenth Century

Initially, I was attracted to the demography of seventeenth-century London because of its association with the origins of demography in Britain. Both John Graunt and Gregory King—the two major pioneers—paid special attention to London. Indeed, Graunt's work is almost wholly focused on London and the Bills of Mortality, while much of the empirical basis of King's analysis also relates to that city.[1] London is also particularly attractive because it was a major metropolitan center in the seventeenth century and has not ceased to be one today. A study of continuity and change might, thus, represent a valuable contribution to the understanding of urban demographic history. More practically, in accepting an invitation from the London Record Society to write an introduction to *London Inhabitants Within the Walls, 1695*,[2] I found that the basic data for London at the end of the seventeenth century are very extensive and offer possibilities for detailed analysis. What began as a small-scale trial was extended to include an analysis of nineteenth-century primary materials.[3]

As to the work itself, three main lines are being followed, two of which relate to data collected under the 1694 Act (6 & 7 Wm. & M., c.6). The stated purpose of this Act was to help finance the war against France by levying taxes on marriages, births, and deaths, and annual dues on bachelors above the age of twenty-five and on childless widowers. As this Act has been discussed in detail elsewhere, it will perhaps be sufficient to say that it specified two kinds of demographic data—namely, a complete enumeration of the inhabitants, with indications of marital condition and socio-economic status and a record or births, deaths, and marriages distinct from, though not completely independent of, the normal parish registra-

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tion of baptisms, burials, and marriages. The Act had a national coverage, but so far the statistical returns found relate to relatively few areas. The returns for London are the most comprehensive and, as compared with other seventeenth-century official returns, seem to have been collected and compiled with unusual conscientiousness. Some of the enumerations, indeed, go beyond the requirements of the Act and list occupations. The Act was in force for ten years, and there were supposed to be annual revisions of the enumeration. Only two revised enumerations have been found for London, although revisions have been found for other areas—Southampton, Shrewsbury, and Lyme Regis, for example. For London, therefore, the basic population analysis has been confined to the data for 1695, while the analysis of vital events has been based largely on the period 1696-98. The third line of work relates to a slightly earlier set of returns—those of the 1692 Poll Tax (under the Act of 3 Wm. & M., c.6).[4] These returns list the occupations—and they appear to be real occupations and not simply guild designations—of a large part of the employed population of London.[5] The administrative basis is that customary for seventeenth-century taxation—namely the wards—whereas the basis of the 1694 Act returns is the parish. It is possible, however, to obtain a fairly close match between the two sets of areas by using precincts within the wards. Although individuals and households are not being matched between the two "enumerations," demographic and occupational structure can be shown for the same parishes, approximately three years apart.

The analysis of the primary materials is time-consuming. Partly for this reason—though also because a rather less detailed analysis of the 1694 Act returns for the whole group of London parishes had already been undertaken by P. E. Jones and A. V. Judges[6]—the study is being restricted to a sample of parishes within the Walls. The City of London was divided into four approximately equal geographical segments; within each segment, a 1-in-2 sample of parishes was selected from an alphabetically ordered list, yielding a total of forty parishes out of the ninety-seven within the Walls.[7] The main analysis has been undertaken for this group. Before discussing the results of that analysis, it is desirable to refer to some of the general characteristics of the City of London, as shown in the earlier, pioneer study by Jones and Judges.

That study covered both the parishes within the Walls and thirteen parishes without the Walls. On the basis of the 1694 Act

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enumeration, the total population was estimated to have amounted to almost seventy thousand within the Walls and almost fifty-four thousand for the thirteen parishes without, a total of 124,000. This is a much smaller population than that given by Gregory King, but his estimate of 528,000 (including an allowance for omissions) covered three additional parishes without the Walls, fifteen parishes in Middlesex and Surrey, and seven parishes in Westminster. The returns for these additional areas—save for a fragment of a parish in Westminster—have not so far been discovered, and King's own journals list the summary data (houses and total population) for only a few of them. The grand total would be for the contemporary equivalent of a conurbation, the East End of London being a large element in it—with a population of some eighty thousand, according to King's own figures.[8] Thus, the Jones and Judges data, and still more those for the parishes within the Walls, describe the core of London, an area which almost certainly declined in population by the beginning of the nineteenth century, and perhaps had even shown some fall after the middle of the seventeenth century. At the same time, London was an area with a fairly distinctive pre-industrial ecology. The proportions of upper-status households were higher in the center, and the lower-status households showed the greatest relative frequency on the periphery and in many of the parishes without the Walls. This kind of pattern was still visible in the London of the 1830's, but it was changing by the middle of the nineteenth century. Taking the sample of forty parishes within the "core" of London, some of the data derived from the enumeration in 1695 are summarized in Tables 1 and 2.

Table 1

City of London Within the Walls, 1695. Population in a Sample of Forty Parishes (absolute numbers)

Total popn.	Houses	Persons liable to surtax	Children	Servants	Apprentices	Lodgers
28,507	4724	4733	6526	5805	755	2475

The first table gives an indication of some of the social characteristics of the total population. In this analysis, socio-economic status is classified on a simple twofold basis—whether the individual in question was liable to the standard rate of tax or to a surtax. The surtax itself was graduated in accordance with a person's wealth or

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Table 2

City of London Within the Walls, 1695. Demographic Structure of a Sample of Forty Parishes (absolute numbers)

	Single			Married	Widowed	Total
	Under 25	25 & over	All			
Males	7221	1350	8571	4392	273	13,236
Females	—	—	9915	4418	929	15,262
Total	—	—	18,486	8,810	1202	28,498*

* Excludes 9 persons of unknown sex.

standing in the society. The lowest rate was paid by individuals with a personal estate of £600 or £50 per year, and the highest by dukes and members of their immediate families (wives, sons, and daughters). It would, however, have been cumbersome to distinguish the gradations. Even the simpler classification used shows a considerable range of variation within the city, from just under 5 per cent of the population liable to surtax in the poorest parishes (St. Mary Staining and St. Andrew by the Wardrobe) on the northwest and southwest edges of the city respectively, to almost 40 per cent in St. Matthew Friday Street, one of the central parishes in the southwest quadrant. Taking the sample as a whole, 16.6 per cent of the population came within the surtax category, counting dependents as well as heads of families.

Some of the headings in Table 1 are not entirely unambiguous. "Children," for example, covers not only individuals described explicitly in that way in the enumeration, but also others referred to as sons and daughters. The number of "children" per house cannot be regarded as an index of relative fertility, as it might be to some extent if there were a definite and consistent age reference to the term "child." "Servants" and "apprentices" are also not entirely distinct categories. It is likely that the term "apprentice" was used fairly rigorously. In the parish of St. Mary le Bow, for example—a district of relatively high socio-economic status, with substantial numbers of merchants and professional people—the sixty-nine apprentices are listed clearly. In that parish, too, the internal evidence suggests that most of the servants were domestics. That is, however, not the case for all the parishes, and some of the servants may well be assistants or individuals serving in something comparable to an apprenticeship role. This perhaps partly explains the inconsistencies in the ratios of servants per house found among the different parishes. Not all the high-status parishes have the highest ratios of

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servants per house. If the parishes of high socio-economic status are taken as a group—covering both the four parishes in the general sample with the highest percentage of the population liable to surtax and the three highest among the remaining parishes within the Walls—the ratio of servants per house is higher than the average for the forty parishes in the general sample. The ratios of persons and children per house are also higher, as may be seen from Table 3. The picture will be clearer when the data have been more fully analyzed, for the analysis will then include a study of individual houses and households within the parishes.

Table 3

City of London Within the Walls, 1695. Some Social Characteristics of Total Sample of Forty Parishes and Seven Parishes with Highest Proportions of Population Liable to Surtax

	Percent of popn. liable to surtax	Persons per house	Children per house	Servants per house	Servants & apprentices per house
1. Total of sample parishes	16.6	6.03	1.38	1.27	1.43
2. 7 parishes with highest percentages in surtax categories*	34.4	6.68	1.46	2.18	2.37

* The total population of these seven parishes is 2797.

Table 2 shows the composition of the population by sex and marital condition, and in Table 4 these absolute figures are expressed as percentages and are compared with similar data for the group of high-status parishes. The situation of London as a whole (that is, the geographical region covered by the census) is also shown in 1851, this point of time having been chosen because urban mortality and fertility were both high then.

The table suggests a greater difference between the higher-status parishes and the general sample in 1695 than between the general sample and the characteristics of London as a whole in 1851. The proportion of unmarried men and women, in particular, is strongly influenced by the social status of the area—this being in considerable measure a reflection of the proportion of servants and apprentices, almost all of whom were unmarried. For example, in St. Mary le Bow, a parish with 35 per cent of its population liable

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Table 4

City of London Within the Walls, 1695. Demographic Structure of Total Sample of Forty Parish and of Seven Parishes with Highest Proportions of Population Liable to Surtax
(*Percentage distribution by marital condition*)

	Males						Females				
	Single			Mar-	Wid-			Mar-	Wid-		Ratio
	Under 25	25 & Over	All	ried	owed	Total	Single	ried	owed	Total	M/100
1. Total sample of parishes	54.6	10.2	64.8	33.2	2.1	100.1	65.0	29.0	6.1	100.1	87
2. 7 parishes with highest percentages in surtax categories	55.1	15.5	70.6	27.0	2.3	99.9	70.8	25.9	3.4	100.1	97
3. London 1851 Census	50.5	10.1	60.6	36.1	3.3	100.0	58.6	32.6	8.8	100.0	88

to surtax and with a ratio of just over two servants and apprentices per house, none of the apprentices and only two of the servants (both men) were married. This heavy weighting by the unmarried must, of course, be taken into account in interpreting the crude birth rates of large cities. Servants and apprentices were also likely to be relatively short-term migrants. Some of them must certainly have settled in London. It is not improbable, however, that, in general, they gave rise to a significant turnover of the population and to that extent promoted contact and communication between London's inhabitants and those in other parts of the country.

Where revised enumerations under the 1694 Act have been found, it may be possible to measure this turnover and perhaps also to obtain some indication of the kinds of people involved in it. For London, unfortunately, only two revised enumerations (for 1696) have been discovered, and one of them is for a parish (St. Peter, Paul's Wharf) for which there is no 1695 enumeration in the Guildhall archives. As an illustration of possibilities and problems, however, the other parish (St. Benet, Paul's Wharf) is of interest. Between the two enumerations, the population of the parish appeared to increase from 562 to 670 persons, a net growth of just over 19 per cent. A comparison of the individuals named in the two enumerations shows, however, that behind this net balance there was an apparent loss of 75 of the people listed in 1695 and a gross intake of 183 people between 1695 and 1696. The losses are equivalent to just over 13 per cent of the 1695 population and the gross gains to about

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27 per cent of that population. A yearly turnover of this magnitude certainly appears extremely high. More information is available about the out-migration than the in-migration, for the 1695 enumeration gives occupations for most of the inhabitants. Thus, of the seventy-five out-migrants, sixteen were servants, nineteen were men (and their families) in what would seem to be migratory occupations (painters, masons, a lieutenant, and a sailor), and three were widows who might, perhaps, have moved to be with relatives. In addition, the deaths of four people (two children and two adults) were traced in the parish register. Thus, in respect to forty-two of the seventy-five losses, the statistics do not seem implausible. The figures of apparent in-migration between 1695 and 1696 are much more difficult to check. Of the total of 183 newcomers, nine were traced through the 1695 list of inhabitants as being in other parishes in the city at the time of the first enumeration, while twenty-six were servants and might thus have been genuine newcomers. The 1696 assessment for St. Benet, Paul's Wharf, is a copy in the handwriting of Gregory King, and King himself lists as "additions" the names of 106 persons—possibly these were, in his view, real additions to the population since 1695. If so, then 143 out of the 183 new names can be explained in not too unrealistic terms. It is, of course, no less possible that there was more undercounting in the first enumeration of 1695 than in that of 1696. King, who appears to have undertaken—or to have sponsored—a post-enumeration check in St. Benet, Paul's Wharf in 1696, notes forty-three persons as having been omitted and six as having been counted twice. There is, however, no comparable evidence of the volume of omissions in 1695. Before we can reasonably evaluate population shifts of the kind suggested by the data for this particular parish, more work will clearly need to be done, especially on the 1694 Act assessments for cities other than London.[9]

In the introduction to *London Inhabitants,* I gave examples of two other types of analysis of the 1694 Act data, one concerning household size and structure and the other bearing upon the degree of completeness of vital registration in London. Nothing further will be said here on the first type, for the rather detailed study of household structure in a subsample of fourteen parishes is not yet complete. The attempt to estimate the completeness of birth and death registration, however, has now been extended to cover a much larger number of parishes, and it is thus possible to make a rather more confident statement about the results.

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In essence—though this was not realized until after the first trials were made—the method used is that proposed in 1949 by C. Chandrasekar and W. Edwards Deming for estimating the "true" numbers of births and deaths by means of surveys in areas lacking effective vital registration.[10] Given two independent surveys, each recording births or deaths in a given area in a specified period and listing the names of the individuals born or dying, it should be possible to compare the names of the individuals in the two surveys. Taking births as an example (if PR refers to one survey and CR to the second), a comparison or matching of individual names and specified characteristics would produce the following results: names included in both PR and CR, which we may call (PR, CR); names in PR, but not in CR; and names in CR, but not in PR. If the surveys are independent, the best estimate of the "true" number of births would consist of (PR, CR) + PR + CR + an allowance for births that escaped recording in both surveys, which we shall call E. Then $E = (PR \times CR)/(PR, CR)$. The technique would apply equally to recorded deaths.

While the 1694 Act was in force, two sets of vital records were available—the normal parish registration entries and the special returns compiled by the collectors appointed under the Act, which levied duties on births, burials, and marriages. Each set listed and named the individuals. Although matching is not easy, it seems generally to be possible so far as the London returns are concerned. This matching has been done for all those parishes in the general sample for which both collectors' returns and parish registers are available, namely thirty-eight parishes within the Walls. In addition, two of the parishes without the Walls (St. Bartholomew the Great and St. Bartholomew the Less) were treated in the same way.[11] Since the Act did not come into force until mid-1695 and may well have encountered initial difficulties, it seemed better to begin with the second full year of operation. Thus, the returns and parish-register entries for the three-year period—May 1, 1696, to April 30, 1699—were used.[12] The main elements in, and the results of, the analysis are summarized in Table 5. The estimates of "true" numbers of baptisms and burials are those given in the column headed "Total"; the last two columns in the table give the ratios by which it was found necessary to multiply the numbers recorded in the collectors' returns and in the parish registers in order to "inflate" them to the "true" totals.

Three conclusions seem to follow from the analysis. First, both

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Table 5

Components Used in Estimating "True" Numbers of Baptisms and Burials in the City of London, 1696–1698

	PR, CR	*CR*	*PR*	*E*	*Total*	Inflation ratios *Total/CR*	*Total/PR*
[A] *Baptisms*							
i) 38 parishes within the Walls	2324	779	770	258	4131	1.331	1.335
ii) 2 outer parishes	228	34	55	8	325	1.240	1.148
[B] *Burials*							
i) 38 parishes within the Walls	2253	611	1058	287	4209	1.470	1.271
ii) 2 outer parishes	265	16	33	2	316	1.125	1.060

registration systems were subject to serious omissions in the parishes within the Walls, the collectors' returns showing especially large omissions in respect to burials. Secondly, omissions seem to be much less serious in the two outer parishes, the parish registers appearing to be rather better than the collectors' returns both for burials and for baptisms. Finally, when the "corrected" totals of baptisms and burials are used, the balance of births and deaths appears more favorable than is often assumed to have been the case for London. It is true, of course, that these estimates relate to a period subsequent to that in which John Graunt was writing and after bubonic plague had ceased to play a significant part in mortality in England. It is possible—and is at least worth investigating—that for some time after the 1660's London was far less a "consumer of men" than is generally assumed, though the position may have worsened considerably with the development of smallpox mortality in the eighteenth century.

In considering these suggestions, it is important to acknowledge that the two systems of vital registration were unlikely to have been completely independent of each other, and that this assumption in the calculation is unrealistic. Nor was one system completely dependent on the other as is evident from the elements in Table 5. The error involved in the assumption is small in relation to the actual "non-matching." If an allowance were made for this error, the "inflation ratios" would have to be increased slightly. What is

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more important—and again this needs further study—is that parish registration may have been still less complete, both before and after the period during which the 1694 Act was in force, because the Act and its subsequent amendments provided for specific improvements in the existing parochial system, both in respect to coverage and to speed of registration. In addition, insofar as the two systems of registration were not independent, each might have benefited from the other. Even with these limitations, it is at least useful to have some definite basis for looking at the reliability of ecclesiastical registration in earlier and later periods.[13]

It is convenient to bring this "report on progress" to an end here. The analysis is continuing, and in the final report it may be possible to include material for communities outside London.[14] If so, there will be some hope of providing better estimates of the level of fertility and mortality for England in the late-seventeenth century than have so far been available, though to do so will not only involve tests of the completeness of registration, but will also require attempts to estimate under-enumeration in the 1694 Act assessments.[15]

REFERENCES

1. My earlier work on Gregory King is reprinted in D. V. Glass and D. E. C. Eversley (eds.), *Population in History* (London, 1965), Ch. 8, while that on John Graunt will be found in a paper on "John Graunt and His *Natural and Political Observations*," *Proceedings of the Royal Society*, Series B, No. 974 (10 December 1963).

2. London Record Society Publications, Vol. 2, Hon.-General ed. W. Kellaway (London, 1966).

3. In carrying out this trial, T. R. Gourvish acted most ably and conscientiously as my research assistant. When the study was extended, he supervised the work of extraction, which was undertaken by a group of graduate assistants during the summer of 1966. He has been continuing to help me (in collaboration with Miss A. Davies) in the analysis of the material. I am greatly indebted to him. I should also like to express my particular indebtedness to Mr. P. E. Jones, Deputy Archivist to the City of London Corporation, for his unfailing help at every stage of the study.

4. *Statutes of the Realm*, Vol. 6 (London, 1819), pp. 302-308.

5. Some of the London returns under the 1694 Act also list occupations, and the final report on this study will contain an analysis of that material.

6. P. E. Jones and A. V. Judges, "London Population in the Late Seventeenth Century," *Economic History Review*, Vol. 6 (October, 1935).

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7. The returns are available for eighty of the ninety-seven parishes; no trace has been found of the missing seventeen returns, and no copies have been located in other archives.

8. For the whole area of Tower Hamlets, King gives a figure of 19,443 houses and a population of 86,882.

9. P. Laslett's analysis of the population changes in Clayworth and Cogenhoe suggests a very high turnover in these two rural areas in the seventeenth century. He finds, in particular, that 62 per cent of the inhabitants of Clayworth in 1688 had not been there in 1676. See P. Laslett, *The World We Have Lost* (London, 1965), p. 147.

10. "On a Method of Estimating Birth and Death Rates and the Extent of Registration," *Journal of the American Statistical Association*, Vol. 44 (March, 1949). A useful summary with reference to the application of this technique to current estimates in Pakistan is given by W. Parker Mauldin, "Estimating Rates of Population Growth," *Family Planning and Population Programs*, eds. B. Berelson *et al.* (Chicago, 1966), pp. 642-47.

11. For the remaining outer parishes, there are problems arising from the differences in areas covered by the collectors' returns and the parish registers.

12. In a few cases, collectors' returns were available for only two of the three years. In such cases, the events recorded have been multiplied by 1.5. Foundlings were included, but not those individuals listed as normally resident in other parishes.

13. The completeness of the data in the London Bills of Mortality is also being investigated, though there are special difficulties regarding the statistics of christenings. These data, too, were not independent of the parish-registration system and, hence, may also have improved during the period in question.

14. I have made some provisional estimates of completeness of registration in Southampton during the period, but hope to amplify them. It should also be possible later to carry out similar computations for Shrewsbury. In the work on London, I shall also be drawing upon other data to which my attention was drawn by Mr. P. E. Jones, namely the records of apprentices who acquired their freedom from the City of London. These records indicate previous place of residence and would thus prove of considerable value in studying the migration of a select group of young men.

15. When Dr. E. A. Wrigley saw this paper in June, 1967, he wondered whether the apparent differences in completeness of vital registration between the inner and outer parishes of the City of London might be a function of the relative populations of these parishes. The inner parishes have small populations; the outer, large ones. For a resident in a small parish, the possibility of recording a birth or death at a church in a nearby parish might obviously be quite high, yet the matching described in this paper had been undertaken only within the same parish.

    To answer Dr. Wrigley's question, the matching process for one year

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was greatly extended. The entries for the sample parishes were matched against the parish registers and collectors' returns for all the London parishes for which the records were available. The detailed findings will be published later. The general result was that the inflation ratios derived from this extended matching did not differ significantly from the original ratios. It would seem, therefore, that in the City of London vital events were very largely recorded in the parish of residence.

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PIERRE GOUBERT

Legitimate Fecundity and Infant Mortality in France During the Eighteenth Century: A Comparison

THIS ARTICLE proposes to study the birth and death rates of children in France during the eighteenth century in order to discover whether they were strictly identical. Since 1962, research has been conducted on Brittany, a populous province which had more than two million inhabitants by 1700. Brittany is a unique province, with its own language (Celtic in the western half), a long history of independence (until the sixteenth century), special legislation (the common law of Brittany until the Revolution), a semi-autonomous spirit, a powerful nobility and gentry, and a curious economy which was both agrarian and maritime. The historian, and the demographic historian in particular, will be interested in the province's great fecundity and Catholic fervor, supported and commanded by the numerous priests, who were popular as well as imperious. The clergy, strongly controlled by the episcopal administration, exercised the greatest strictness and seriousness in drawing up the parochial registers after the sixteenth century. Fortunate circumstances, like Brittany's remoteness from war zones, have insured the excellent preservation of these basic documents. The registers of baptisms, marriages, and burials are entirely preserved in numerous localities from 1600 until 1792, and sometimes even from a date prior to 1500. Moreover, the influence of Protestantism was weak, limited, and well defined in Brittany; thus, for almost every period, the Catholic parochial registers concern the total population. Since the seventeenth century at least, the deaths of children of low age were also registered, which was not the case in all of France, especially the south.

With these numerous and excellent sources, the method of family reconstitution can be applied, according to the principles estab-

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lished by Michel Fleury and Louis Henry.[1] This method consists of reconstituting the complete demographic history of a married couple from the birth of either of the two spouses until the dissolution of the household; it includes the birth and death of their children. If the parochial registers are complete, and if the family remained in one location, the results obtained by this method are accurate and will satisfy most of the demographer's curiosity.

I have encouraged three students at Rennes to study localities in the interior of the Haute-Bretagne (upper Brittany), according to the method of Henry. Jacqueline Caro worked on La Guerche for the period 1733 to 1792 (which represents approximately four thousand inhabitants); Jean Besnard, on Saint-Méen Le Grand for the period 1720 to 1792 (approximately eighteen hundred inhabitants); and Michel Renouard, on Saint-Aubin d'Aubigné for the period 1740 to 1789 (approximately one thousand inhabitants). These three large parishes, which now belong to the *département* of Ille-et-Vilaine, are within thirty or forty kilometers of Rennes—the first to the south, the second to the west, and the third to the north of the city. Caro, Besnard, and Renouard have drawn up more than thirty thousand individual case histories and have succeeded in reconstituting more than fourteen thousand peasant families of the eighteenth century.

The first characteristic of fecundity in Brittany is not surprising to the historical demographer who is familiar with old France: Nearly 99 per cent of the births are legitimate. Of 14,947 registered baptisms, only 201 concern illegitimate children. This rate illustrates the pressures exerted by both Catholicism and society and is not exceptional for the heart of the provinces, where the situation of the unmarried mother was exceptional and difficult. The rates of illegitimacy were much higher in the cities and the ports. Never, however, do they reach the high figures one finds in England during the seventeenth and eighteenth centuries. In this period, virtue is French, even in the lower classes.

The second characteristic is no less surprising: Women start to have children very late, seldom before their twenty-sixth or twenty-seventh year. In other words, the age at marriage is high. In the whole century and in all three localities, the average age at first marriage for women is always higher than twenty-five years; at Saint-Aubin d'Aubigné, it is twenty-seven years. Men are usually two to four years older than their spouses. This phenomenon,

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rather common in the old peasant France, has never been fully explained. It might be related to the age at which a person reached his majority—twenty-five years in most of the provinces, although there is no general rule for the entire country. Young men or women did not fully enjoy their civil rights until they reached age twenty-five, when they could inherit property, acquire goods, and appear in court, and paternal power remained strong for a long time. In Brittany, a marriage could not take place without paternal authorization; even after age twenty-five, one had to ask for it, if only out of politeness. Before age twenty-five, young people could only exist within the security of the family.

Women started to have children late, and they also stopped early. The cause of this cessation was physiological. At the time of their last delivery, women were usually between forty-one and forty-three years of age. After age forty, fecundity is weak; after forty-five, it is practically nonexistent.

What is the rhythm of births during this rather short period between a woman's twenty-sixth and forty-second year?

About 6 per cent of married couples are sterile for reasons that are unknown, but can be assumed to be physiological. In rural France during the eighteenth century, about half of the young women gave birth for the first time less than a year after their marriage; about 75 to 80 percent, within less than eighteen months.

The study of other intervals gives the following results:

Interval between births)	*La Guerche*	*St.-Aubin*	*St.-Méen*	*St.-Méen*, women with a "rapid rhythm" (*the third part*)
1–2	18.6 months	18.2 months	20.2 months	15.3 months
2–3	19.5	19.9	21.6	15.1
3–4	21.2	21.4	22.6	17.1
4–5	21.0	25.0	25.0	19.4
Before the last:	27.5	26.5	25.8	19.8

These intervals are by far the shortest of those that have been observed in eighteenth-century France.

Using a technical method to correlate observations based on 1,047 families, we can show the legitimate fecundity by age groups. No other part of France shows so high a rate as Brittany. In the eighteenth century, only the results from Canada and Sainghin-en-Mélantois, a village in the north of France, are comparable,

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Women Aged	*20 to 24*	*25–29*	*30–34*	*35–39*
St.-Aubin:	0.582	0.520	0.489	0.358
La Guerche:	0.507	0.487	0.458	0.270
St.-Méen:	0.582	0.548	0.476	0.385

although they do not reach the same levels.[2] By isolating that third of the women at Saint-Méen le Grand whose childbearing was most rapid—those aged twenty to thirty-four—one arrives at a rate between 0.775 and 0.669, or three to four children in five years.

The fecundity of these women does not diminish in the course of the eighteenth century. When one isolates those women at Saint-Méen le Grand that married between 1720 and 1755 from those that married between 1756 and 1792, one discovers with surprise that the latter are the most fecund (at twenty to twenty-four years, 0.632, as against 0.560; at thirty to thirty-four years, 0.502 as against 0.464).

Other methodologies exist by which one can present this fecundity, which is both exceptional for France and rising. One can, for example, multiply by 5 the sum of the fecundity rate for women twenty to forty-nine years of age, as Henry did for Sainghin-en-Mélantois.[3] One obtains 11.1 for Saint-Méen for the first half of the eighteenth century and 12 for the second half. Not a single calculation given by Louis Henry exceeds 10.8. These figures sensitively express the average number of children per family for women that married at age twenty and lived in the marriage state until age fifty.

The following are the real numbers of children in those families that were studied at Saint-Méen: 52 per cent of the families had less than five children; 36 per cent had between five and nine children; and 12 per cent had ten to twenty-one children.

If one reverses the methodology, taking the children as the starting point, one obtains the following results: 21 per cent of the children born at Saint-Méen came from families with less than five children; 50 per cent from families that had five to nine children; and 29 per cent from families with ten or more children.

The legitimate fecundity in the three Breton villages is exceptionally high and does not diminish, but, on the contrary, rises during the second half of the century. There does not seem to have been the slightest trace of birth control in that province. These three Breton villages do not, however, exemplify all of eighteenth-century France; a comparison is necessary for the wider picture.

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For this comparison, I shall take two examples, most unlike those in Brittany, from the southwest of France—the one is from a study by Pierre Valmary, published by the I.N.E.D. (Institut National d'Etudes Démographiques) on Thézels and Saint-Sernin in Bas Quercy[4] and the other is from a thesis, as yet unpublished, by Anne Zink, on Azereix near Tarbes at the foot of the Pyrenees. I shall also use the famous monograph by E. Gautier and Louis Henry on Crulai, a Norman village.[5] To simplify the account, I shall use only the intervals between births (the number of births within five years of marriage) and births classified according to the age of the women, both of which are likely to be good indicators of marital fecundity.

In terms of months, the following are the intervals between births:

	Thézels	*Azereix*	*La Guerche (Brittany)*
Interval 1–2	25 months	29 months	19 months
Interval 2–3	30	31	20
Interval 3–4	32	33	21
Interval 4–5	32	32	21
Before the last	31	37	28

In Brittany, the intervals between successive births are close to twenty months; in the southwest, they are close to thirty months. Such a marked difference cannot be the result of chance.

The number of births (given in five-year intervals), when correlated to the age of the women, yields observations of the same kind:

Age of women:	*20–24 years*	*25–29 years*	*30–34 years*	*35–39 years*
St.-Méen, 1756–92	3.16	2.95	2.51	2.00
Crulai, 1674–1742	2.33	2.19	1.68	1.49
Thézels, 18th cent.	1.89	1.82	1.51	1.13

The contrast between the parish in Brittany (St.-Méen) and the parish in Quercy (Thézels) is obvious. The rates for Crulai (Normandy) are intermediate, but still closer to the distant Quercy than to the bordering Brittany.

This comparison underlines the great differences between the exceptional fecundity of the Breton women and the rather weak fecundity of women in the southwest. Other monographs are necessary, but if the given examples prove to be truly representative, one must ask what caused such a wide disparity.

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Two hypotheses, and two only, seem to me to be possible. We are either confronted with two types of women, with two types of couples, whose biological characters differ profoundly, or with two types of behavior, two types of society or civilization. In Brittany, couples accept all the children "that God sends them"; in the southwest, they do not. In Brittany, the Catholic priests exercised strict control on private life, especially through confession; in the southwest, this control was weak. In Brittany, birth control was probably not practiced until the end of the eighteenth century; in the southwest, a sporadic and intermittent limitation (*coitus interruptus*?) was practiced in order to delay the succession of births.

It is usually held that rural districts in France passed from a state of uncontrolled births to one of controlled births during the eighteenth century. Thus far, this affirmation has been proved for only four villages, all of which are north of Paris—Sainghin and the three villages in the Ile-de-France studied by M. Ganiage.[6] Results for Brittany and the southwest do not confirm such a rapid transition. In these provinces, there seems to have been no change in birth-control practices from one end of the eighteenth century to the other. In Saint-Méen le Grand, fecundity increased, rather than decreased.

In a France that was divided by differences in language and law, the diversity and regional contrast prevailed, maintained in part by a poor transportation system. The real eighteenth century, as far as rural and provincial populations are concerned, remains to be discovered. We must descend from gossipy generalities to humble realities.

Does the study of infant mortality in Brittany and the southwest confirm these observations? (It is difficult to know the mortality of adults because of their mobility.)

The ecclesiastics that kept the parochial registers in Brittany were particularly attentive in inscribing the deaths of very young children, even those that died immediately after birth and the stillborn. The reasons for this attention were based in religious conviction. In contrast, the ecclesiastics of the south were much less careful. Frequently they did not even inscribe in their registers the deaths of children under ten years of age. Since these children had not taken communion, they were considered to be "incomplete" Christians. Moreover, the south was much less devout than the north. M. Valmary himself could not use the parochial registers of

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Thézels and Saint-Sernin before 1746 in his study of infant mortality there.

The Breton figures, therefore, are much more reliable than the figures of the southwest; the Breton rates are certain and have been calculated from five to ten times as many cases as the others that have been published for France thus far.

The following are the results for Brittany's infant mortality (the proportion of children that died during their first year):

La Guerche (1720–92)	285 out of 1,000
Saint-Méen (1720–92)	237 out of 1,000
Saint-Aubin (1749–89)	243 out of 1,000

Roughly one child in four died during his first year. This rate is higher than what I calculated a short time ago for Beauvais between 1665 and 1735.[7] Certain historians found this rate exaggerated, but did not provide acceptable arguments for their criticism.

The rates for the southwest and Crulai were:

Thézels (1747–82)	191 out of 1,000
Azereix (18th century)	156 out of 1,000
Crulai (1674–1742)	172 out of 1,000

Roughly one child out of every six or seven died during his first year. One discovers this rather low rate in some villages in the region of Paris, in Normandy, and in the south.

Thus, the notion of "infant mortality" is far from satisfying. The grave dangers to young children do not cease one year after they are born. The difficulties of weaning are added to others. Of much more utility are the rates of child mortality from zero to four years, or from zero to nine years. I shall content myself with giving the rate at which children survive until age ten. Beyond ten years, the children have often moved and cannot be easily traced. Furthermore, after age ten a child is less threatened.

Children Surviving to Age Ten in Three Breton Localities

Saint-Aubin (1748–89)	580 out of 1,000
La Guerche (1720–90)	510 out of 1,000
Saint-Méen (1720–92)	463 out of 1,000

Roughly half of the children do not reach their tenth year. The situation does not improve in the course of the eighteenth century. At the beginning of the second half of the century, the rate of

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survivors at Saint-Méen retreats from 475 to 451. This retreat corresponds to a slight increase in fecundity.

Children Surviving to Age Ten in the Southwest and Normandy

Thézels	645 out of 1,000
Azereix	639 out of 1,000
Crulai	672 out of 1,000

Nearly two thirds of the children reach their tenth year in these areas. The two groups, thus, vary in their rate of infant mortality as well as their rate of marital fertility.

The phenomenon of mortality is perhaps more complex than the phenomenon of fecundity, as more factors are involved. The demographic explanation of the demographic phenomenon of mortality is not satisfactory. It is only one point of view, one aspect, one element of the total picture; it is not sufficient by itself.

A comparison between the interior of Brittany and the interior of Aquitaine may prove useful. The natural conditions are not the same. Upper Brittany has neither cold winters nor sunny summers, and the climate there is very humid. A lot of water becomes stagnant, and the months of August and September are stormy, dull, and wretched. Thus, the majority of children die in summer: The poor quality of water, the swarms of parasitic insects, and the lack of hygiene probably played their role in the high death tolls during the summer. The southwest seems to offer a more healthful climate, one that is more sunny. Did these climatic differences count for much in the eighteenth century?

After the end of the seventeenth century, the two regions under consideration were spared the great famines which struck other French provinces. In both cases, a diversified agriculture sheltered them from poverty and famine. From this point of view, the superiority of the regions of Aquitaine is striking. Since the seventeenth century, the cultivation of corn assured the population of enough food. The numerous secondary cultivations—grapes, fruit, trees, poultry, and vegetables—enabled the population to maintain a level of subsistence through trade with other regions. Inner Brittany fed itself solely on buckwheat pancakes, salty butter which was often rancid, salty lard, and black bread. People drank bad cider, and too much hard cider, which they sometimes gave (and still give) to children. The filth of the Breton farms, although proverbial, is not apocryphal: the well next to dung, dung in front

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of the door, animals in the house. Moreover, the quacks, the matrons (midwives without any qualifications), and the sorcerers of the villages aggravated the situation in Brittany. The southwest seems to have been less dirty and less superstitious.

The economic and social conditions also acted in the opposite direction in the two regions. The rights of the feudal lords and aristocrats always weighed heavily on the peasants of Brittany. Moreover, the rural development was limited, dispersed, and insufficient. The south of France was less subject to seignorial and "feudal" dues, but the extent of peasant cultivation is not known.

These are only possible explanations for the contrast between the very high infant mortality in Brittany and the more moderate mortality in the southwest. The exact causes are still unknown.

What do these observations concerning two French provinces during the eighteenth century teach us?

It is certain that the Bretons were ignorant of all birth control during the entire eighteenth century. It is likely that the married couples in the southwest practiced some birth control throughout the century, although not systematically. In neither case can a significant change be observed, nor is there any evidence of systematic contraception aimed at limiting the total number of children, rather than the interval between births. There are considerably more differences between regions than between periods.

This observation holds true for infant mortality: It is high in Brittany and does not diminish in the course of the eighteenth century; although much weaker in the southwest, it maintains a constant level throughout the century.

Neither Brittany nor the southwest represents all of France. The area around Paris constitutes a quite different case, as it was shaken by great demographic crises during the seventeenth and part of the eighteenth centuries. (These became attenuated about 1750.) Such phenomena are the exception in Brittany and the southwest. The area around Paris is characterized by a rather high fecundity, one clearly much stronger than that of the southwest, but perceptibly weaker than that of Brittany. Around Paris, one can observe a slight decrease in mortality during the eighteenth century and the beginning of systematic contraception after 1780.

Are there, then, several "demographic regimes" in the interior of the kingdom of France, several types of behavior, or even several types of civilization that demographic analysis can help to reveal?

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Analysis of the demographic, economic, and social phenomena of ancient France can be undertaken only for regions. Nothing has as yet been determined with any degree of certainty, and historians must stop teaching unfounded ideas about comprehensive demographic development, which has not thus far been sufficiently explored.

One can object that although our comparison dealt with two disparate elements, they produced the same results. Indeed, the Breton families had a lot of children, but lost a lot of them. The families in the southwest had fewer children, but did not lose so many. Do these conditions not produce the same results? If they do, it is a disturbing situation.

A little reflection is in order. One hundred Breton families in the three localities considered here produced 460 children; of these, about 230 reached adulthood. One hundred and twenty of the 230 were women. To maintain the total population, it was necessary for one hundred of these women to marry, given the same demographic conditions. The rate of celibacy for women wavers between 10 and 15 per cent, and the rate of sterility for married women is close to 6 per cent. These summary, and partly hypothetical, calculations lead to the conclusion that the Breton population could not grow during the eighteenth century. On the other hand, it is known that between 1770 and 1787 the number of deaths exceeded the number of baptisms by more than one hundred thousand.[8] The demographic documentation is not so precise for the southwest. Nevertheless, historians often support the thesis that the southern population increased after 1740, although so far this has not been established.[9] It is fortunate that Louis Henry could prove that the rural population in the area around Paris underwent a strong growth after 1750, because proof for the other regions is missing or not very convincing.[10]

Is it necessary to doubt the theory that French population increased during the eighteenth century? I do not think so. It is merely necessary to examine the phenomenon very closely—for the course of the century, from one province to another—in order to demonstrate without doubt that the subject is more complicated, more diverse, and more varied than it was thought to be for a long time. As a Danish prince said: "There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy." Much work remains for the historical demographer.

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BERNARD H. SLICHER VAN BATH

Historical Demography and the Social and Economic Development of the Netherlands

Studies in economic and social history have advanced rapidly since 1950; not only have they used new methods and techniques, but they have dealt with problems that have changed radically. During the first stage in the development of studies in economic history, each sector of economic life was treated separately, thus producing histories of agriculture, commerce, industry, banking, and so forth. In the second stage, the separate histories were joined on a national scale, but still not integrated. The internal development of each of the segments remained divorced from the other parts of economic life, resulting in the segmentation characteristic of the well-known textbooks of economic history—the economies of the United States, England, the Netherlands, and so on. Since 1950, however, the various economic activities have come to be considered part of an economic and social totality. We now know that a change in one of the segments causes repercussions in many other parts of the economy and the social structure. The ideal equilibrium of the old economic theories is constantly disturbed. From a historical point of view, the disturbing factors are particularly interesting.

Before 1800, the greatest changes occurred mostly in the population figures and the relations among prices. Increase or decrease of population affected both production and consumption. Growth of population is not always correlated with an increase of productive forces because the age distribution and the occupational structure can change. Moreover, different dietary and spending habits within the social stratification can influence consumption. Before 1800, many people spent most of their income on food.

In integral economic history, based on a holistic viewpoint, the

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most important problems of the pre-industrial period have become demographic changes and the price relations of goods and wages. Problems are now studied which remained unexplored in the older segmental national economic histories. Quantification, especially of population and prices, is the backbone of new researches in the history of economic and social development.

The researches on the Netherlands being done by the Department of Rural History at Wageningen University focus on the period between 1500 and 1800; the statistical data, therefore, are often lacking or incomplete. Each investigation relates to a definite territorial and administrative unit—a province or a major part of a province. Prior to 1800, each province had its own administration because the Republic of the United Provinces was a federation. Thus, diverse source materials are available for the various provinces. A regional treatment is also justified because a great variety of patterns existed in the various areas between 1500 and 1800.

The regions investigated so far are widely distributed over the country: Friesland, studied by J. A. Faber; Holland, by A. M. van der Woude; the Veluwe by H. K. Roessingh; the bailiwick of Bois-le-Duc by H. J. van Xanten; and Overijssel, by myself. The investigations cover 56 to 66 per cent of the total population living between 1500 and 1800 on the present territorial area of the Netherlands.

The investigations are focused on long-term developments, so that considerable attention is paid to reconstructing the evolution of the absolute number of inhabitants. Other aspects of the demographic past are also taken into account—birth rate, death rate, migration, age distribution, fertility, age at marriage, nuptiality, family size, family structure, the distribution of the population among the various religious denominations, demographic differences among the religious groups, and so forth. In regard to some of the latter aspects, the work has sometimes met with great difficulties owing to the extensive migration and religious diversity within the Dutch population.

Demographic investigations form only a subsidiary part of the study, which embraces the entire economic and social life of the area concerned. Attention is paid to such different phenomena as the occupational pattern, the distribution of wealth, the social stratification, agriculture, rural industries, navigation, fishing and peat-digging, the incidence of taxation, drainage duties, ground rent, and the prices of land and produce.

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The Sources

Sources used for demographic analyses were general and local population censuses, reports of the number of communicants, lists of men capable of bearing arms, reports of the numbers of hearths, homes, or families, tribute registers, poll-tax registers, revenues of taxes on such necessities as corn and meat, and, finally, parish registers. All of these sources present peculiar difficulties in interpretation. The first true national census dates from 1795-96; previously, incidental censuses had been taken in the various provinces, mostly for purposes of taxation. The results of these censuses are often imperfectly preserved, and only fragments are available.

In order to infer the total number of inhabitants from the number of communicants, men capable of bearing arms, and hearths, homes, or families, it is necessary to have conversion coefficients. These are related to such factors as age structure, ratio of males to females, and average size of the household or family. In interpreting the tax registers, the evasion factor must obviously be taken into consideration.

The perusal of parish registers is beset with other kinds of pitfalls. Because the registers of deaths and burials are scarce, this important aspect of the demographic development generally falls outside our scope. Few registers of baptisms and marriages antedate 1650. Such registers as exist relate mainly to the congregations of the Reformed Church. It is questionable if they represent the trend in the total number of inhabitants. Unfortunately, the baptism registers of the Roman Catholics usually begin after 1700 and the registers of births of the Mennonites in the second half of the eighteenth century. Moreover, the Roman Catholic missionary stations and the Mennonite congregations usually cover a wider area than do the Reformed Church congregations, the latter being limited chiefly to a single town or village. There exists a possibility of double registration of the Roman Catholics, for their baptisms and marriages should have been entered in the Protestant registers, although Catholics backed out of this obligation when possible.

Even when the available baptism statistics are representative of the birth rate of the entire population, it is still an open question as to what extent the trend in the number of births is an index of the trend of that population. Strictly speaking, this would only be the case if the birth rate remained stationary. Obviously there are an-

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nual fluctuations in this figure. Owing to changes in the marriage rate, the marriage age, natural fertility, female mortality, and intentional interference with marital fertility, differences may also occur over a longer period.

G. J. Mentink and A. M. van der Woude have used parish registers in their study of the demographic development of Rotterdam during the seventeenth and eighteenth centuries. The Amsterdam registers of bridegrooms and brides will be a rich source for investigation. Researches into these registers have only been made for migration and the marriage age of brides, but the registers also contain other interesting information, such as data on occupations and literacy.

Family reconstitution, a method being used by Louis Henry for France and the Cambridge Group for England, is complicated in the Netherlands because many people among the lower classes had no family names before the introduction of civil registration in 1811. They were called by their Christian name combined with the genitive of their father's Christian name. Sometimes the Christian name was linked with the name of a trade or a farm.

For the economic and social history, use is made of a host of new records, many of which have not been previously studied. Although differing in value according to the investigation concerned, important sources are assessment lists and related materials from which data have been deduced about wealth, occupation, acreage of land under cultivation, balance between crop farming and cattle breeding, size of the livestock population, number and size of farms, balance between leased and owned property, extent of poverty, industry, and tax revenues; also used are weigh-house records, rent accounts, administration of dike contributions, data on land reclamations and utilization, peat-digging, cattle plague, dispensation of taxes, village accounts, and deeds in notarial files. Publications in the field of price history are also most pertinent.

The Demographic Development

It is possible, using the 1795-96 national census and the knowledge of the development in the five areas studied (Friesland,[1] the Veluwe,[2] Overijssel,[3] the bailiwick of Bois-le-Duc,[4] and Holland[5]), to calculate the hypothetical number of inhabitants of the present territorial area of the Netherlands from 1500 to the present day:

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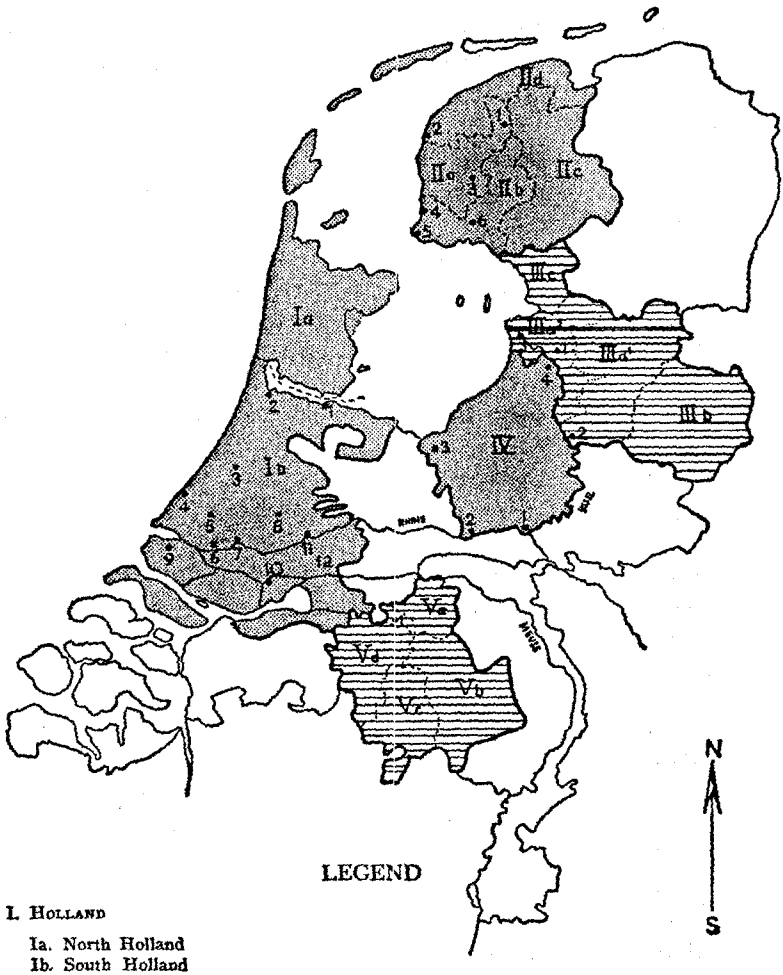

LEGEND

I. Holland

Ia. North Holland
Ib. South Holland

1. Amsterdam
2. Haarlem
3. Leyden
4. The Hague
5. Delft
6. Schiedam
7. Rotterdam
8. Gouda
9. Den Briel
10. Dordrecht
11. Schoonhoven
12. Gorkum

II. Friesland

IIa. clay pasture district

IIb. peat pasture district

IIc. "Wouden"

IId. clay arable district

1. Leenwarden
2. Harlingen
3. Sneek
4. Hindeloopen
5. Staveren
6. Sloten

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III. OVERIJSSEL

IIIa. Salland
a¹. arable farming area
a². livestock farming area

IIIb. Twente

IIIc. Vollenhove

1. Zwolle
2. Deventer
3. Kampen

IV. VELUWE

1. Arnhem
2. Wageningen
3. Nijkerk
4. Hattem

V. TOWN AND BAILIWICK OF BOIS-LE-DUC

Va. Maasland
Vb. Peelland
Vc. Kempenland
Vd. Oisterwijk

1. Bois-le-Duc with its freedom

Although the figures relating to periods prior to 1795 are hypothetical and provisional, their calculation is not entirely a matter of guesswork. Only 35 to 40 per cent of the calculations were made

Population of the Netherlands, 1500–1950

Year	Population	Year	Population
1500	900,000 to 1,000,000	1750	1,900,000 to 1,950,000
1550	1,200,000 to 1,300,000	1795	2,078,000
1600	1,400,000 to 1,600,000	1850	3,057,000
1650	1,850,000 to 1,900,000	1900	5,104,000
1700	1,850,000 to 1,950,000	1950	10,200,000

solely by means of geographical interpolation, the remaining 60 to 65 per cent being based on sufficiently reliable factual material.

There is great regional and chronological variety in the demographic development of the five areas studied, as is shown in the following table of the absolute numbers of inhabitants and the indices and percentages.

Total Number of Inhabitants of the Five Areas

Area	ca. 1500	ca. 1650	1795
Friesland	75,000	145,000	162,000
Veluwe	36,000	41,000	66,000
Overijssel	53,000	71,000	135,000
Bailiwick B.	96,000	82,000	117,000
Holland	275,000	903,000	783,000
Total	535,000	1,242,000	1,263,000

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Indices of the Demographic Development of the Five Areas

Area	ca. 1500	1795	ca. 1500	ca. 1650	ca. 1650	1795
Friesland	100	216	100	193	100	112
Veluwe	100	186	100	114	100	163
Overijssel	100	255	100	134	100	190
Bailiwick B.	100	122	100	85	100	143
Holland	100	285	100	328	100	87
Netherlands	100	219	100	197	100	111

	Indices 1650–1795				Percentages 1500–1795		
Area	ca. 1650	ca. 1700	ca. 1750	1795	ca. 1500	ca. 1650	1795
Friesland	100	90	93	112	7.9	7.7	7.8
Veluwe	100	121	132	163	3.8	2.2	3.2
Overijssel	100	117	172	190	5.6	3.8	6.5
Bailiwick B.	100	134	138	143	10.1	4.4	5.6
Holland	100	98	87	87	28.9	48.2	37.7
Other Provinces	—	—	—	—	43.7	33.7	39.2
Netherlands	—	—	—	—	100.0	100.0	100.0

A turning point occurred about 1650. Between 1500 and 1650, demographic growth predominated, especially in Holland and, in a lesser degree, in Friesland. The growth was not so spectacular in Overijssel and the Veluwe. In the second half of the sixteenth and the first half of the seventeenth century, a decline set in in the bailiwick of Bois-le-Duc, owing to the Eighty Years' War (1568-1648). The eastern parts of Overijssel also suffered badly during this war.

The period from 1650 to 1795 shows considerable growth in Overijssel and the Veluwe, moderate growth in the bailiwick of Bois-le-Duc, but stagnation in Friesland and decline in Holland. The growth in the bailiwick took place between 1650 and 1700, a recovery after the Eighty Years' War. In Overijssel, much of the growth occurred during the first half of the eighteenth century, and in the Veluwe during the second half of the same century. In Friesland, a decline between 1650 and 1700 was followed by a recovery during the second half of the eighteenth century. Stagnation characterized the second half of the seventeenth century in Holland; the decline appeared in the first half of the eighteenth century.

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There is a decline in population after 1650, especially along the coasts of the Zuiderzee—in North Holland and the coastal areas of Friesland and Overijssel.

The Economic and Social Conditions

It is difficult to explain the many changes in population numbers. To what extent was the growth of population a consequence of a natural increase, of immigration? Was the decline in population caused by higher mortality, by emigration, or by a lower marriage rate? Extensive migration took place inside the country. People from the countryside came to the parish-villages and the neighboring towns. Amsterdam attracted people from the whole country, and great numbers came from the villages and towns in North Holland. The republic was an asylum for persecuted people from other countries: Calvinists, Huguenots, Lutherans, Mennonites, and Jews. The prosperity of the Netherlands lured some who sought a change in fortune; still others came as soldiers. Numerous persons also emigrated to the East and West Indies, South Africa, Brazil, the New Netherlands, Scandinavia, Germany, and Eastern Europe; we do not know the extent to which emigration counterbalanced immigration.

There are no signs that mortality was higher after 1650 due to the spread of contagious diseases. We know of ten plagues and three other epidemics between 1500 and 1650; during the period from 1650 to 1800, there were four plague attacks and six epidemics. The last attack of the plague was in 1668-70. The plague had, therefore, already disappeared when the population in Holland and the Zuiderzee area started to decline.

The country suffered from many wars, but its heaviest losses were incurred during the Eighty Years' War. The southern and eastern parts of the Republic endured particularly severe hardships in this struggle. Despite such heavy costs, the country reached the culminating point of its Golden Age during the last phases of this war. In most parts of Holland, the population continued to increase; in Overijssel and the bailiwick of Bois-le-Duc, population growth began again. The succeeding wars did not result in heavy losses of human lives.

Between 1650 and 1800, inundations became more frequent. There were nine inundations between 1500 and 1650 but nineteen after 1650. Agricultural prices were low during this period, and the

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costs of maintaining the dikes and polders were high. There were many complaints against high dike taxes, which weighed heavily on the rural population. The inundations were most probably the result of the dikes being inadequately maintained. During this period, the piles at the foot of the sea-dikes were destroyed by the teredo plague, and large sums were required to replace these wooden defenses with stone ones, chiefly imported from Norway.

An increase in population always coincides with new economic possibilities in agriculture, industry, shipping, and peat-digging, whereas a decline in population is attended by recession in trade, agriculture, and industry. It is impossible to solve the problem of causality: A growing population means expansion of production and consumption, and new economic possibilities create opportunities for population growth. A decreasing population results in the restriction of production and consumption, and a decline in economic possibilities leads to a decrease in the marriage rate, a higher age at marriage, and more celibacy. Once the process has started, there is always a reinforcing secondary effect. The process of population growth can be observed in the province of Overijssel after the Eighty Years' War. Agricultural potentialities were exploited by resettling abandoned farms and fields. Further land clearance was restricted because wastelands were a source of heather sods and humus for the arable lands (turf-manuring). When the price of cereals dropped, the textile industry relieved the burden of imminent overpopulation. At first, the cottage weavers lived among the farmers. Eventually they broke away from this environment and became professional weavers, settling mostly in certain villages and towns. The various wars fought between 1688 and 1748 were exceptionally favorable to the sale of textile products.

The Veluwe is an area with a continued modest growth in population. Before 1650, during the period of relatively high and rising cereal prices, the reclamation of wastelands kept pace with population growth. Buckwheat cultivation was expanded on the higher lands, thereby enabling a greater number of persons to subsist on the same acreage. During the depression in arable farming between 1650 and 1750, the expanded activity of the tobacco, paper-making, and textile industries led to a substantial increase in employment. The economic situation for the industries and the tobacco growers became less favorable after 1750, but the appearance of the potato corrected it by opening up new prospects for smallholders.

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A classic Malthusian pattern is enacted in the neighboring eastern part of Overijssel. A rise in population was founded on the small base of the textile industry. After nearly a hundred years of population growth, the general state of health, diet, housing, education, and medical and spiritual care had deteriorated to a low level. After 1750, the keen foreign competition led to a structural crisis in the textile industry. Universal poverty resulted, and the growth in population came to an end.

The dependence on one industry may be at the root of the small resistance this region had against economic misfortune. Still, no region was hit harder by the economic depression and decline in population than North Holland with its shipping centers, fisheries, livestock farms, and diverse industries along the river Zaan and elsewhere. A many-sided economic development, thus, does not guarantee the uninterrupted continuation of prosperity. The whole economic life of North Holland was closely connected with shipping and foreign trade, mainly with the Dutch shipping in the Sound. The Baltic trade flourished from 1550 to 1650, owing to the increasing demand for cereals in Western and Southern Europe. After 1650, the situation changed. The general stagnation in population growth had a negative effect on the demand for cereals. Moreover, Southern and Western Europe were beginning to produce more because of the increase in acreage devoted to rice, maize, and buckwheat. The steep decline in the demand for Baltic cereals adversely affected Dutch shipping in the Sound, a change fatal to the situation in North Holland.

In general, a region that had more economic opportunities was also more dependent on the market and economic fluctuations. The economic diversity in the Netherlands perhaps postponed, but certainly did not prevent, the penetration of the general European depression in the seventeenth century.

Various possibilities for economic expansion were available to the Netherlands. Agriculture could have been developed in all five districts; peat-digging combined with shipping and industry, in Friesland and Overijssel; shipping, in Friesland and Holland; sea fishery, in Holland; industries connected with trade, shipping, and export, in Holland; textile industries, in Overijssel, the bailiwick of Bois-le-Duc and the Veluwe; and papermaking, in the Veluwe and Holland.

Between 1500 and 1800, periods of economic rise and depression alternated. There was a rise from 1500 to 1650, a depression

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from 1650 to 1750, and then another rise after 1750. During periods of economic expansion, the ratio between the price of grain and the prices of other commodities and wages was favorable to high grain prices. The ratio was unfavorable during the depression period.

Many new polders were made in Holland between 1500 and 1650, and thousands of people worked to drain the land. There were also reclamations in Friesland, the Veluwe, and the bailiwick of Bois-le-Duc. Reclamations started again on a more modest scale after 1750.

The high prices of wheat and rye stimulated the cultivation of other crops: buckwheat in the Veluwe in the sixteenth and seventeenth centuries, and the potato in Friesland, the Veluwe, Overijssel, and the bailiwick of Bois-le-Duc after 1750. Because buckwheat required no direct manuring, it could be grown as an unmanured last crop in the rotation after one or two rye harvests. Such a crop was important in view of the continual lack of manure. Buckwheat cultivation had a further advantage because it countered the great weed infestation resulting from the cultivation of rye as a single crop. Checking the weed infestation served to increase the rye yield. Thus, the expansion of buckwheat cultivation had a multiple effect: The same amount of manure was put to better use, there was more intensive land use, and the rye harvest was improved. All these factors meant that a greater number of persons were able to subsist on the same acreage, thereby creating conditions conducive to an increase in population.

During the depression period (1650-1750), farmers were interested not in growing grain, but in cultivating cash crops. Tobacco was grown in the Veluwe; flax, coleseed, and hops in Friesland and the bailiwick of Bois-le-Duc. The rural industries prospered during this period: The textile industry developed in Overijssel, the Veluwe, and the bailiwick of Bois-le-Duc, and papermaking flourished in the Veluwe. In all of these regions, the rural industries met with difficulties after 1750, when the economic situation began to improve.

As far as can be seen, peat-digging was not subject to economic alternations. In Friesland, low transport costs made peat comparatively cheap, thereby favoring the establishment and development of industries in which fuel was a relatively important cost factor—for example, brickworks, tile-works, salt-works, lime kilns, breweries, and distilleries.

In northwest Overijssel, peat excavation represented an impor-

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tant means of employment. Peat transport led to a great expansion of shipping and, in turn, to the development of many other branches of industry, such as shipbuilding, sail-making, rope and shoe manufacturing, and baking. Peat-digging, however, like gold-digging, brings only temporary prosperity. Once the peat has been removed, all that remains are lakes and ponds. When this district provided no further livelihood from peat, it was abandoned and the population sought fresh possibilities in the peat reserves of Friesland and Drenthe.

Although the depression was part of a general European phenomenon, in the Netherlands it was aggravated by domestic problems. The government contracted large loans to finance the many wars in the second half of the seventeenth and beginning of the eighteenth centuries. The tax burden was increased to meet the interest on and discharge of these loans. The economic situation, however, demanded a decrease in taxation. The fiscal system also worked in the wrong direction, as can be seen in Overijssel. During the period of low grain prices, high taxes were imposed on agriculture. After 1755, when grain prices began to rise, the highest tax incidence shifted from agriculture to the principal foodstuffs. This measure further aggravated the condition of the textile workers, who were already suffering from underemployment due to crises in the textile industry.

A Malthusian Example

Exact figures are only available for one province of the Netherlands. These data refer to the increase in population of the province of Overijssel. Between 1675 and 1764, the population nearly doubled: The index was 100 in 1675, but 186.9 in 1764. All the social and economic phenomena observable at that time point to a relative overpopulation—the occupational structure, social stratification, housing conditions, medical and spiritual care, education, and financial and agricultural development.

In 1795, less than half of the occupational population worked in agriculture (45.6 per cent). The importance of industry is a striking feature of the occupational structure; 35.5 per cent of all workers were employed in industry and nearly half of them were in the textile trades. Commerce (11.6 per cent) and social services (5.1 per cent) were of less importance.

By classifying the population on the basis of property we can

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discern the numbers of persons belonging to various groups and the total value of properties owned by each group. Between 1675 and 1758, the importance of the nobility declined, and the number of citizens with fortunes of more than 10,000 guilders dwindled. The supremacy of the nobility and city patricians, who had ruled the province for many centuries, was broken. The upper-middle classes, composed of people with fortunes between 500 and 10,000 guilders, grew. This new group included lawyers and doctors, as well as linen manufacturers in Twente. They acted as money-lenders to the nobility, giving mortgages on castles and sometimes even becoming the new inhabitants of castles formerly belonging to the nobility. The lower-middle classes, made up of people with yearly incomes of less than 500 guilders and of paupers who could not pay the low poll taxes, rose enormously. The increasing pauperization was alarming. In 1675, a quarter of the total population was in the pauper class; by 1758, this percentage had risen to 35.7. All cottage weavers in Twente were paupers.

Conditions for the cottagers and weavers were aggravated by poor housing. In towns and rural areas, people lived in turf huts, sheds, bakehouses, and pigsties. Many new "houses" had a surface area of 150 square feet, the height being under six feet. Between 1682 and 1749, the number of new houses built did not keep pace with the increase in population. The net number of houses rose by 3,235, but the number of families rose by 11,000. This lag meant that houses were available for only 30 per cent of the new families.

Poor housing conditions were most probably a factor in the increase in the number of diseases resulting in a high mortality after 1750. Medical care was on an insignificant scale. In 1795, the province had only thirty-four doctors, fifty-four surgeons (barbers), and seventeen midwives, or one doctor per four thousand and one midwife per eight thousand inhabitants. The situation was worse in the rural areas than it was in the urban centers because the doctors and midwives were concentrated in the towns. The same inadequacy characterized spiritual care and education: one Protestant clergyman per 1,600, one Roman Catholic priest per 4,300, and one teacher per 900 inhabitants.

The financial development, as far as we can conclude from the increase in the total amount of property declared for the purpose of property taxes, remained far below the increase in population between 1675 and 1758. The index of the population in 1764 was 186.9, that of 1675 was 100; the index of the total amount of prop-

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erty was only 113.7 in 1758, when that of 1675 was also 100. The increase in the number of paupers unable to pay the poll tax was far greater than the population increase. In some places in Twente in 1764, paupers accounted for 50 to 60 per cent of the total population.

Taxation did not greatly increase in severity, but it was still higher than the increase in property. The index of the tax yield in 1760 was 134.1; in 1680, it was 100. It is understandable, therefore, that the government shifted the burden of taxation from property to excises on foodstuffs. The property taxes were paid only by landowners, the excise taxes by the whole population. This measure created great difficulties for the weavers, the peat diggers, and paupers. A high consumption of brandy was an accompanying feature of the universal poverty. The yield from excises levied on strong alcoholic drink was one of the province's chief sources of revenue, representing 16 per cent of the total tax yield between 1770 and 1790.

The agricultural population did not increase so rapidly as the total population: Index for the agricultural population in 1795 was 162.6 (1602, 100) and for the total population, 189.7 (1675, 100). There was an even greater lag in the growth of the acreage of arable land and pasture: In 1749, the respective indices were 106.4 and 104.2 (1602, 100). As a result, the farms became smaller and the cotter holdings more numerous. The number of horses used as draught animals on the farms remained practically unchanged between 1602 and 1812. This fact indicates that there were not more medium-sized farms in 1812 than there had been in 1602. The increase in arable acreage took place in the small cotter holdings. Pig-keeping is characteristic of cotter farming, and the number of pigs increased greatly between 1602 and 1812. There was also a decline in the number of sheep between 1602 and 1844, due mainly to the decrease of wasteland during the first half of the nineteenth century.

Before 1800, there were few possibilities for land clearance because the wastelands were used for grazing livestock and as a source of humus. In the first half of the nineteenth century, the acreage of cultivated land increased more rapidly than the population. The cultivation of turnips and fodder plants ended the dependence on earth manuring with heather sods and sand. The cattle population increased, and more pigs were kept. Potato cultivation also brought great change, by increasing the yield per acre.

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During the period of its population increase, Overijssel depended on the import of cereals from other areas to supply the towns, villages, and rural districts with food, because it was unable to sustain its population on its domestic yield. Nor was the increase in livestock proportional to the growth of the human population. As a result, sufficient grain and animal proteins were unavailable to the population, which must have had an adverse affect on their health. A medical report of 1781 relates that the cottage weavers of Almelo were weak and lethargic.

The divergence between the growth of the agricultural population and the increase in the acreage of arable land and pasture produced great changes in the social stratification of the rural population. In 1602, the farmers still formed the largest group in the agricultural population (61.4 per cent), but by 1795 they were less than half (46.9 per cent); the cotters maintained their relative numbers (about 35 per cent). Owing to the steep rise in population, no additional living space was available for large numbers of country dwellers, even as cotters. A large group of day laborers developed, amounting to 18.3 per cent of the entire agricultural population by 1795. An almost landless proletariat sprang up, its sole resources being physical strength and the capacity for work. An increasing differentiation in the social stratification is observable among the rural population. The social scale tended to a large downward spreading.

All these phenomena confirm the picture of a society under the heavy stress of a too rapid increase in population. After 1764, the population growth came to a standstill, due in part to higher mortality. The age of marriage may have risen, and more people may have remained unmarried. In the nineteenth century, many three-generation households are found in the province of Overijssel; there are few indications of such households in the Census of 1748, taken during the period of growth.

In some parts of the province, hereditary laws were in force which stipulated that only one of the children in a family could inherit the farm. This law was applied to the farms of the villeins from the Middle Ages until 1795. It may be that after 1750 the custom was expanded to other farms in some villages. With the exception of the heritable child, children often remained unmarried. Researches into these problems, however, are still to be done.

H. K. Roessingh has published many exact data for the Veluwe, especially for the situation in mid-eighteenth century. We hope to

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get a better understanding of the interaction between changes in the population and in the social and economic conditions for the other parts of the Netherlands, but this work is still in progress.

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1. In Friesland, the increase before 1689 occurred in the towns, the center and southwest of the province, and in the sandy regions to the east; the decline after 1650 hit the towns and the livestock-farming area in the west of the province. The renewed growth was less pronounced in the clay pasture area and the towns.

2. In the Veluwe area, there was a steady, continuous increase in population which was free from any drastic changes. During the sixteenth century and the first half of the seventeenth, the growth was modest, although the rate of growth gradually increased. Between 1650 and 1750, the increase occurred mainly in the rural areas. The urban population showed little increase during this period, the only marked growth being in the small towns of Nijkerk and Wageningen where tobacco cultivation and trading rose to prosperous heights. After 1750, the stagnation in urban growth came to an end, and the rate of growth in the rural area remained at practically the same level.

3. Overijssel suffered considerably as a result of the Eighty Years' War, during which there was fierce fighting, particularly in the eastern part of the province. The country districts were plundered and laid under ransom. A good many persons emigrated from these districts to Amsterdam and other towns in Holland during this period. When peace was restored, a surprisingly rapid increase of the population set in, mainly between 1675 and 1748, especially in the eastern part. The growth occurred chiefly in the rural areas and hamlets during the first period from 1675 to 1723. Between 1723 and 1748, the increase was most striking in the parish-villages and the towns. The rapid increase in population between 1675 and 1748 was due to the excess of births over deaths, as appears from the large number of children. The increase in the eastern part is paralleled by that in the adjoining German districts, the bishoprics of Münster and Osnabrück. In the western part of the province, the increase was far less spectacular, much more regular. This area is very similar to the Veluwe on the other side of the river IJssel.

   In some towns and villages on the Zuiderzee coast, the population even declined, just as it did in areas of Holland and Friesland, which were situated along the Zuiderzee.

   The pattern underwent a complete change during the second half of the eighteenth century: In many parts of Overijssel, the population remained stationary or even declined.

4. In the bailiwick of Bois-le-Duc, the population increased very rapidly during the first half of the sixteenth century. The growth was particularly marked in the pasture districts and the centers of the textile industry—Tilburg, Helmond, Eindhoven, and their surrounding districts. The growth was less marked in the high lands with sandy soil.

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An industrial crisis in the cloth industry, religious struggles, the loss of trade routes, and finally the Eighty Years' War contributed to a rapid decline in population of the town of Bois-le-Duc—falling from 23,000 in 1526 to 9,000 in 1665. In the rural districts, the war had less disastrous results. Characteristically, the number of inhabitants in the countryside was as high and probably even somewhat higher in 1665 than it was in 1526. The western part gained 15 to 20 per cent, but the pasture district lost a few per cent.

After the war, the town of Bois-le-Duc saw a considerable growth in population, reaching almost 14,000 in 1747. After that date, the number of inhabitants continued at the same level for the remainder of the century. From 1665 to 1766, there was a slight growth in the rural population. During these hundred years, the bailiwick underwent a period of rise between 1665 and 1700, of decline between 1700 and 1750, followed by some revival after 1750.

5. In Holland, a period of continued growth began with the early-sixteenth century and lasted for one hundred and fifty years. In the first stage, between 1514 and 1569, the growth was focused particularly in the countryside and the smaller towns. The expansion of Amsterdam, Leyden, Haarlem, Delft, Rotterdam, and other towns did not start in earnest until after 1580. Toward the end of the sixteenth century, the countryside of Holland was becoming so overpopulated that many countrypeople could no longer make a living there and had to go to the cities; moreover, the stream of immigrants, mainly from the southern Low Countries and Germany, was directed to the larger cities. The growth of the towns at the end of the sixteenth and the beginning of the seventeenth century far exceeded that of the countryside. In 1514, more than 46 per cent of the population of Holland lived in towns; this percentage was 54 in 1622.

After 1650, the population in the area north of Amsterdam began to decrease—slowly at first, but catastrophically after 1700. In this area, the numbers of inhabitants decreased from more than 200,000 in 1650 to about 130,000 in 1750. In large parts of North Holland, the decrease even came to 50 per cent.

In the area of Holland south of Amsterdam, which was the largest part and where most of the inhabitants lived, the demographic development was not uniform. There was a stagnation or even a slight decrease in the countryside. With exception of Amsterdam, Rotterdam, the Hague, and Schiedam, the towns saw their population decrease, often considerably. Their demographic development can be divided into three phases: from 1622 to about 1680, there was continued growth; between 1680 and 1750, a rapid decrease; and between 1750 and 1795, a slow decrease. The total decrease between 1650 and 1750 for the whole province of Holland may be estimated at about 120,000 persons.

The causes of the decrease are not yet clear. Many possibilities exist—such as a higher mortality, a lower average number of children per marriage, a higher age at marriage, and an increase of celibacy. In Amsterdam, the age of brides at first marriage was higher in the eighteenth century than in the seventeenth. In 1626-27, 51.5 per cent of the brides were twenty to

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twenty-four years of age; in the eighteenth century, this figure was 30.6 to 32.4 per cent. Many brides were thirty to thirty-four years old: 18 per cent in the eighteenth century, against 7.3 per cent in the seventeenth. During the eighteenth century, there was a very remarkable stagnation in the Protestant denominations, while the Roman Catholics increased, or did not decrease to such an extent. Statistics for Rotterdam, Delft, and some other towns indicate that Protestants had fewer children per marriage. At Rotterdam, Protestants had, on an average, one child less per marriage than did the Roman Catholics during the eighteenth century.

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SUSAN B. HANLEY

Population Trends and Economic Development in Tokugawa Japan: The Case of Bizen Province in Okayama

DESPITE THE existence of national population statistics dating as far back as 1721 and evidence of striking changes in population trends during the pre-modern period, until recently little interest has been shown in Japan's demographic history. Sources for the study of the pre-modern population in Japan are far better than those for most other countries, but many of these have either been ignored or used inefficiently. The traditional view of population trends in this period has been based on unadjusted Tokugawa government figures; until the last three or four years, the potentially rich village sources remained virtually untapped.

During the Tokugawa period (1600-1868), Japan's population almost doubled, going from an estimated eighteen million to around thirty-five million. Rapid urbanization accompanied this marked increase in population. Edo (now Tokyo), the administrative capital of Japan, was a village in the early-seventeenth century, but by the eighteenth and nineteenth centuries, it was probably the largest city in the world, containing between five hundred thousand and one million inhabitants. The population of the urban area surrounding the commercial center of Osaka was nearly as large. These two cities did not, however, constitute isolated phenomena; large administrative and market centers grew up around the castle town of each Japanese domain.

The usual image of the Tokugawa population has been one of rapid growth in the first half of the period with a subsequent leveling off of the population at around thirty million.[1] The rapid growth in the first century has been attributed to the economic development that took place in a Japan at peace and unified after

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years of civil war. When the Tokugawa house gained hegemony over the contending *daimyo* and set up its administration in Edo in the early-seventeenth century, it initiated a number of policies which were to have far-reaching effects. Foreign trade and intercourse ceased except in amounts too small to have much economic effect, and Christianity was prohibited. The supply of reclaimable land dwindled during the two hundred years in which Japan was isolated, both politically and economically. These two factors have been used to explain the leveling off in the population growth rate. In essence, Malthusian theory has been applied to Tokugawa Japan.

According to the stereotype of Tokugawa population, the Malthusian limitations at work in a closed economy kept the population constant after its period of growth and subsequent leveling off. An increasing interest in modernization and the preconditions for modern economic growth have, however, led to new research on the population of Tokugawa Japan.

Excellent sources exist for the study of Tokugawa population. The Japanese showed an earlier and greater interest in the quantification of economic resources and the significance of population numbers than did Europeans. A strong government coupled with a large bureaucracy made fairly accurate surveys possible. In 1721, the shogunate ordered an enumeration of the population of the domains comprising Japan; these surveys were then made at regular intervals until the mid-nineteenth century. Surveys, dating from the late-seventeenth century, were also taken of various domains with differing degrees of depth and accuracy.

The domain (*han*) statistics were compiled basically from village records. In certain areas, registers enumerating the households, persons, and draft animals in a village were drawn up as a kind of census. More prevalent are the *shumon aratame cho*, the religious investigation records. These surveys were first undertaken in the seventeenth century as a means of preventing the practice and dissemination of Christianity. The documents purport to record the name, age, relationship to head of household, and temple affiliation of every person in a village. Since they are fairly thorough, their reliability can be evaluated by examining the internal consistencies. Their reliability is greater for the earlier years, however, because Christianity soon disappeared for the most part, thus obviating the original religious purpose of the surveys. The function of the surveys gradually shifted to an assessment of population.

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The first national survey of population, conducted in 1721, was based on existing surveys; the order for the census stated specifically that no new survey was necessary.

For those villages for which a continuous time series exists, the records can be analyzed by the method of family reconstitution, without the painstaking work of assembling families from scattered baptismal, marriage, and burial registers.[2] Unfortunately, with the exception of one area in Kyushu, not enough records exist to permit an entire area to be analyzed in this way.

Traditional views of Tokugawa population trends are being revised and modified, according to information gleaned from applying new techniques of analysis to both local records and aggregate statistics. New research particularly calls into question the theory of the stabilization of the population during the second half of the Tokugawa period. If the conclusions of studies on the Meiji period (1868-1912) are pushed back into the Tokugawa period, it becomes obvious that the growth during the latter half of the Tokugawa period must have been sufficient to bring the population of the mid-nineteenth century to the level of the early Meiji period. The survey of 1872, certainly more accurate than those at the end of the Tokugawa period, showed a population of thirty-five million persons, a difference of five million. The political unrest and the lack of any indication of a rapid rise in population in any contemporary records make a rapid growth in the early Meiji period unlikely, if not impossible. Thus, the population growth rate was probably higher at the end of the Tokugawa period than the government records indicate. The growth of urban areas increased social mobility, a supposition borne out by the incomplete recording of city populations.

Aggregate figures for the population of Tokugawa Japan by no means give the entire picture of demographic development and often cloak significant trends. While the national population statistics suggest that the population became stable during the eighteenth century, various regions show vastly different trends. Decreases in population in three regions in central and northern Japan were great enough to counterbalance the large increases that occurred in the outlying regions, particularly in the west. Only the northeast (Tohoku region), which suffered from unusually inclement weather in the eighteenth century, and the regions with the largest urban populations (the Edo and Osaka areas) showed decreases in population according to the official shogunal statistics.[3]

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In order to obtain a clear picture of Tokugawa population trends, it is necessary to focus on a smaller unit than the nation and to supplement available statistics with analyses of existing village documents. The domain of Okayama has proved to be excellent for a regional study, because more sources are available for Okayama than for most areas.[4] Not only do good documents exist for a number of villages, but there are breakdowns of the domain population by district for at least seven different years in the Tokugawa period. For at least one year, there is an analysis of the composition of the population by occupation, including the *samurai* or warrior class, which is one of the hardest groups to pinpoint in terms of numbers during the Tokugawa period. Although Okayama may not be completely representative because of the great differences among Japan's two hundred and fifty-odd domains, it cannot be considered atypical. In the sense that aggregate population statistics tend to hide the important developments taking place, the demographic history of this one domain reflects the national trend. Because of the geographical similarities between Okayama and its neighboring domains located along the Inland Sea, many of the trends and developments in Okayama were typical of the neighboring domains in Southern Honshu.

This report on population trends within Okayama and their implications is only a preliminary one. The statistics used here are those of the domain, primarily the aggregate figures for the various districts. At least four or five villages can be analyzed in depth by the family-reconstitution method. The results of such an analysis will add much information to the existing knowledge, but they will not be available for probably another year. The findings herein may well have to be revised in the future, but the trends are clear enough to warrant the present discussion.

The modern prefecture of Okayama is located about one hundred miles west of Osaka, along the northern shore of the Inland Sea. This prefecture takes its name from the domain of Okayama, located in one of the three major areas from which the modern prefecture was formed. The domain of Okayama was composed of a number of scattered areas, but its basic component was the province of Bizen. Since the other areas in the domain were scattered and their size and population fluctuated over time, discussion of the domain's population will be based on Bizen, which included the basic eight districts and the administrative capital of Okayama.

The population of Okayama domain went from an estimated

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300,000 in the latter half of the seventeenth century, to a peak of over 360,000 in the early-eighteenth century, but then declined to 340,000 in the early-nineteenth century. The figures for the province of Bizen plus the castle town show a similar trend. The figures for the years 1721, 1798, and 1834 listed in Table 1 include persons age two and above. No correction has been made, because there is no evidence that figures for the other years include infants under age two. (Usually, children were not recorded until they reached at least age two.) Moreover, such corrections cannot be made until the results of some village studies on Bizen become available.

Table 1

The Population of Bizen and the Castle Town of Okayama[5]

	Bizen		Castle Town		Total	
Year	Figures	Index	Figures	Index	Figures	Index
1679	244,180	100.0	30,033[a]	100.0	274,213	100.0
1707	284,503	116.5	30,635	102.0	315,138	114.9
1721	338,513	138.6	30,296	100.9	368,809	134.5
1738	304,135	124.6	26,349	87.7	330,484	120.5
1798	320,795	131.4	21,218	70.6	342,013	124.7
1834	318,207	130.3	20,173[b]	67.2	338,380	123.4

[a] The castle town figure is for 1680.
[b] The figure is for 1838, the only year in this period for which there is a figure.

The warrior class is known to have been omitted from these data. Statistics for Okayama on this class exist only for the year 1707, when 10,027 persons were recorded as belonging to it. This figure constitutes about 3 per cent of the domain's population, an extremely low proportion since the estimated average for Japan is 10 per cent. Because the proportion of *samurai* to the rest of the population in a domain was unlikely to have increased after the end of the seventeenth century, the omission of this group from the statistics does not affect basic trends, except in the castle town where most of this class lived.

A breakdown of the population of Bizen into its eight constituent districts gives a very different picture of population trends than do aggregate figures. The total population of the domain followed the same trend as the national population, but trends within Bizen differed considerably. The three districts of Mino, Kamimichi, and Kojima showed a substantial rise in population during the entire period, a remarkable increase for an area which had been well populated for over a thousand years and had a traditional economy. None of the other five districts showed substantial de-

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Table 2

The Population of the Eight Districts of Bizen

District	Mino[6]		Kamimichi		Oku		Wake	
Year	Figure	Index	Figure	Index	Figure	Index	Figure	Index
1679	17,679	100.0	35,021	100.0	42,886	100.0	30,287	100.0
1707	21,558	121.9	38,094	108.8	46,107	107.5	34,040	112.4
1721	51,817	293.1	46,293	132.2	50,561	117.9	35,697	117.9
1726	51,163	289.4	47,456	135.6	51,989	121.2	36,562	120.7
1738	23,444	132.6	39,021	111.4	48,469	113.0	35,432	117.0
1798	40,933	231.5	50,782	145.0	44,599	104.0	32,755	108.1
1834	40,630	229.8	51,354	146.6	44,578	103.9	32,187	106.3

District	Iwanashi		Akasaka		Tsutaka		Kojima		Total	
Year	Figure	Index	Figure	Index	Figure	Index	Figure	Index	Figure	Index
1679	17,815	100.0	27,224	100.0	31,825	100.0	41,443	100.0	244,180	100.0
1707	20,780	116.6	32,478	119.3	39,707	124.8	51,738	124.9	284,503	116.5
1721	21,231	119.2	34,156	125.5	42,816	134.5	55,942	135.0	338,513	138.6
1726	21,536	120.9	34,688	127.4	43,295	136.0	58,379	140.9	345,068	141.3
1738	21,795	122.3	33,651	123.6	42,820	134.5	59,503	143.6	304,135	124.6
1798	17,930	100.6	27,839	102.3	37,342	117.3	68,615	165.6	320,795	131.4
1834	16,223	91.1	26,073	95.8	34,652	108.9	72,510	175.0	318,207	130.3

creases, nor did they increase in any significant way considering the length of the period and the reliability of the statistics.

During the century and a half from 1679 to 1834, the population of Kojima increased by 75 per cent, that of Kamimichi by 47 per cent, but that of Mino by nearly 130 per cent. The figures for Mino are probably seriously in error, since an increase from 21,558 in 1707 to 51,817 in 1721 would have been virtually impossible without some sort of mass migration, and there is no record of any social phenomenon of this magnitude. Nevertheless, population undeniably tended to increase in these three districts.[7]

What accounted for the radical variation in population trends within an area united politically and economically? The two groups of districts differ significantly in geographical location. The three districts which sustained large population increases were in the southern half of Bizen on the Okayama Plain, one of the few broad flat areas in this part of Japan; all three were located on the sea or Kojima Bay and had excellent access to transportation and water facilities. The further north the location within Bizen, the more mountainous it is. (North of the Okayama Plain lie the Kibi Plateau and then the Chugoku Mountains.) Tsutaka, Akasaka, and Iwanashi were located in this inner part of Bizen and had no

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access to the sea. These areas were composed largely of low mountains and valleys, where there was little room for the creation of new fields through land reclamation. In contrast to this, land reclamation was fairly extensive throughout much of the Tokugawa period in Kojima, Mino, Kamimichi, and Oku. Kojima was an island until the nineteenth century, but through reclamation in Kojima Bay it was gradually attached to the mainland. The large amount of land reclaimed from the sea differentiates Okayama from other domains, although land reclamation was carried out during the Tokugawa period throughout Japan.

In Okayama, small-scale private reclamation began in the last quarter of the sixteenth century; large-scale reclamation was initiated in the mid-seventeenth century under the direction of the domain government. Land fronting on the sea was reclaimed in the lower reaches of the three rivers which flowed through Bizen to Kojima Bay. By the time three large-scale projects, one in Oku and two in Kamimichi, had been completed at the end of the seventeenth century, land with a productivity totaling more than five hundred thousand bushels of grain had been added to Okayama domain. From this time on, reclamation lagged, probably because there was no more land that could be easily reclaimed from the sea.

Table 3

Percentage of Cultivated Land and Population of Each District in Total Bizen Figures

Year	Population[a]			Cultivated Land	
District	1679	1726	1834	1702	(1702 in *Cho*[a])
Mino	7.2	14.8	12.8	12.9	2,803
Kamimichi	14.3	13.8	16.1	18.0	3,893
Oku	17.6	15.1	14.0	12.9	2,790
Wake	12.4	10.6	10.1	9.3	2,007
Iwanashi	7.3	6.2	5.1	7.4	1,605
Akasaka	11.1	10.1	8.2	12.8	2,770
Tsutaka	13.0	12.5	10.9	15.7	3,392
Kojima	17.0	16.9	22.8	11.1	2,393
Total	99.9	100.0	100.0	100.1	21,653

[a] One *cho* equal 2.45 acres.

Reclamation projects not only added land to the existing area, but often improved the original land. Irrigation and drainage channels were constructed as part of the seventeenth-century efforts in reclamation. The seventeen-kilometer-long Kurayasu River, com-

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pleted in 1679, provided water for the reclaimed land in Kamimichi as well as for a large area outside the project. Another drainage channel was built in the seventeenth century across the same district to prevent flooding of the castle town. Both projects were of immense importance to Kamimichi as a whole and undoubtedly contributed to its productivity.

The economic development of the districts was also closely correlated to population trends. Although it is impossible at this stage in the research to draw causal relationships, the correlation is too close to be coincidental. The economy of Okayama changed radically during the Tokugawa period. Over 80 per cent of the population was engaged in agriculture in 1707; even at the end of the Tokugawa period, Bizen was overwhelmingly rural. By the nineteenth century, striking developments had taken place in the domain's economy with the development of commerce, pre-modern manufacture, and commercial agriculture on a large scale.

The villages witnessed the intensification of rural trade, increasing progress toward a rural money economy, and the commercialization of agriculture. Cotton, the basis of the area's commercial agriculture, was increasingly cultivated in Kojima, Kamimichi, and Oku,[9] primarily in the newly reclaimed fields. The development of cotton culture began in the mid-Tokugawa period; by 1846, 44.8 per cent of the cultivated land belonging to the five villages in one of the reclamation projects in Kojima was planted in cotton. Rice cultivation did not suffer since cotton could be planted as a winter crop.

The development of cotton cultivation and the commercialization of agriculture fostered the emergence of a cotton textile industry, largely carried on as a handicraft enterprise in the home. Many of these developments took place in Kojima and the areas bordering the newly reclaimed land. This new economic activity would explain Kojima's ability to maintain an increasing population with progressively less land per person.

There are other reasons for increases in population in Kojima. The fishing industry was well developed by the early part of the Tokugawa period. Indeed, in terms of earning power a fishing boat was considered as valuable as owning paddy land. The trading industry expanded, and Kojima became a port center for the coastal trade. The development of trade and fishing as well as commercial agriculture and manufacture stimulated the economy of this region, enabling it to support a larger and growing population on far less

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acreage per person than the other districts. Even as far back as 1702, Kojima had twice as many people in relation to its production of grain as most of the other districts.

Mino and Kamimichi also benefited from the commercialization of agriculture and the development of trade. During the early Tokugawa period, the castle town, the largest market in the domain, was located in this area and controlled virtually all important trade and commerce. Rural commerce was centered in thirteen rural towns maintained by traders based in the castle town. The castle town of Okayama had a registered population of more than thirty thousand in the eighteenth century; it was probably far larger than this, however, because most of the warrior class and their attendants lived there.

By the end of the seventeenth century, traders began to appear even in small rural villages. They were usually men who had farmed on a scale that was not economically viable. With the commercialization of large segments of agriculture and the spread of manufacture and trade to the villages, merchants of the castle town lost their monopoly on commerce, a possible explanation for the decline in the registered population of that city. (By the 1830's, its population was only twenty thousand, or two thirds of what it had been in the early-eighteenth century.) Although this subject needs further study, the center of trade clearly shifted from the castle town to the rural areas, and Mino and Kamimichi were among the first to benefit from this.

The innermost districts of Bizen were not so capable of developing economically or benefiting from land reclamation and the new type of agriculture which it fostered. They created the fewest new fields, showed the least amount of economic change, and sustained the smallest increases in population during the period.

There is little evidence of extensive famine and crop failure, though poor crop years were not unusual. Contemporary records do not mention any widespread epidemics. There were severe famines in Western Japan during the 1730's due to insect pests, but these ended with the discovery that whale oil would kill the pests without harming the crop. These crop failures probably contributed to the drop in population during the 1730's. There were national crop failures in the 1780's and the 1830's, and their effect on the population of Bizen must not be negated. Nevertheless, the population in the most densely populated areas continued to increase even in these periods.

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Population control is one of the most debated subjects in Tokugawa demography. Infanticide and abortion were carried out to such an extent that both the shogunal and domain governments felt it necessary to pass edicts concerning these practices. The domain government does not seem to have resorted to this expedient in Okayama, where its concern centered more on the practice of abandoning fields and villages in order to work in the cities and towns. There is, unfortunately, little information on the extent of migration in Bizen. In the village of Fujito in Kojima, a large number of workers were hired from the surrounding villages for work in Fujito, but a large number of persons from that village also worked in other places.

It is often difficult for the historical demographer to uncover the relationship between population trends and general social and economic patterns. This is true to some extent in the case of Japan. It is extremely clear, however, that the relationship between economic development and population changes in the Okayama domain are too consistent to be coincidental. This correlation between economic development and population trends is not restricted to Okayama. In 1846, the population of the entire country was only 103.23 per cent of what it was in 1721, but the population of the four domains considered the most influential areas in carrying out the Meiji Restoration had increased to 130.25 per cent of the 1721 figure by 1846.[10] This would seem to imply that the areas strong enough to challenge the Tokugawa government were also those capable of supporting the largest increases in population, an indication of strong economies.

To people who are not immersed in Japanese history, the conclusions which can be drawn from the statistics on Bizen and Okayama domain will not seem particularly striking. No definitive statement can be made with regard to many of the most important aspects of population growth and composition. To those steeped in the traditional studies on Tokugawa Japan, however, the difference in growth trends within the province of Bizen is most startling. That aggregate figures hide regional and local differences in growth is a truism, but one too often forgotten in studying a pre-modern period in which documents are difficult to find and even more difficult to interpret. There is a tendency for trends in one area or in the country as a whole to be taken as representative of each area in the country individually. Yet trends differed widely according to region during the Tokugawa period. Okayama exem-

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plifies the diversification of both demographic and economic developments within a fairly small and supposedly integrated area.

Furthermore, these developments have been discerned from a kind of statistic which traditional demographers have largely ignored, and demographers using the most recent techniques of analysis have increasingly scorned. There are certainly many inaccuracies and omissions in the regional statistics. Moreover, because there is no breakdown further than that by sex, it is impossible to determine their accuracy by testing internal consistencies. Nevertheless, these statistics were compiled from the basic village registers and can be presumed to be nearly as accurate as the registers; they certainly reflect the same basic population trends.

These statistics have the advantage of being aggregates of local areas. The documents, lying between the national aggregates and the individual village statistics, can prove to be extremely valuable in filling in gaps in knowledge. Nevertheless, just as aggregates hide differences in trends in areas smaller than the one aggregated, so is there danger in using statistics on a village with a population of four hundred and assuming that the results are representative of the surrounding area. There seem to have been substantial differences in population trends in certain villages in Kojima, for example.

Although demographic research on Bizen and the domain of Okayama has only just begun, a correlation between economic development and population trends is apparent. Further research, including analysis of the four or five villages whose records lend themselves to family reconstitution, should provide much additional knowledge on this subject. Even with the present level of knowledge, however, it is evident that significant changes occurred behind the façade of population stagnation during the latter half of the Tokugawa period. Neither the economy nor the population remained constant. Fundamental economic development took place with the growth of trade and the commercialization of agriculture; the resulting increases in economic production enabled certain areas to support growing populations. The outstanding example is Kojima.

In fact, the domain of Okayama seems to have acquired the preconditions to industrialization: the growth of a money economy, improved communications, the commercialization of agriculture, the penetration of commerce and trade into the rural areas, and the development of pre-modern manufacture. Both the level of

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living and the population size rose in areas where these developments took place. The role played by population in the economic developments prior to industrialization is still a matter for conjecture. Further study of Japan may help to isolate the exact nature of the relationship between economic and demographic phenomena. It may point out what in population trends is significant in terms of industrialization and its preconditions.

The author wishes to express her indebtedness to Professor John W. Hall of Yale University and Professor Sumino Taniguchi of Okayama University, who made their sources available. Their research and guidance form the background from which this study was drawn.

REFERENCES

1. The official figure is twenty-six or twenty-seven million, but some adjustment must be made because the warrior class, usually estimated at about 10 per cent of the entire population, and other smaller groups were not included in population surveys.

2. The work of family reconstitution has been pioneered in Japan by Professor Akira Hayami of the Department of Economics, Keio University, Tokyo. The findings of Professor Hayami have been published in English in the *Keio Economic Studies* and in excerpts in the *Mita Gakkai Zasshi.*

3. The urban population of Tokugawa Japan constitutes one of the great demographic mysteries in Japanese history. It is difficult even to calculate what the approximate populations of these areas were, and the figures quoted earlier of one million are extraordinarily rough estimates. There is much conflicting evidence, though the enormous size of these cities has never been disputed. Part of the problem in estimating the size of Edo (Tokyo) and Osaka is due to the composition of the population. The warrior class and their immediate attendants were not required to register. If they did, they registered not in the city, but in the home domain. As time passed and social mobility increased, there was an increase in the number of unregistered persons living in the cities, or persons who had registered in their own villages in rural areas. During one famine, the government asked persons in desperate need of food to register for relief, and 1.3 million persons registered in the Edo area alone. This figure seems entirely out of proportion, but does indicate the existence of extremely large urban populations in pre-modern Japan. Why they seemed to have decreased in population during the late-eighteenth and nineteenth centuries is unknown. In the case of Osaka, the decrease seems to have been related to a change in the commercial structure in the direction of a decentralization of markets.

4. For maps of this area during the Tokugawa period, see Richard Beards-

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ley, John W. Hall, and Robert E. Ward (eds.), *Village Japan* (Chicago, 1959), especially p. 38.

5. The Bizen figures for 1679 and 1834 listed in Tables 1 and 2 are from the *Gogungun Mononaridaka narabi ni Ninzu Sono Ta* and the *Ninzu Cho*, respectively, and are quoted in Sumio Taniguchi, *Okayama Hansei Shi no Kenkyu* (Research on the History of the Domain of Okayama; Tokyo, 1964), pp. 460-61. The remaining Bizen figures were compiled from documents from Okayama in the collection of Professor John W. Hall. The 1707 figure is from the *Arinin Aratame Cho*, that for 1721 from the *Ryochi Tahata Chobu Ninzu Cho*, and those for 1738 and 1798 from the *Ninzu Aratame Cho*. The castle town figures were obtained respectively from the *Uenin Sodate Mokuroku*, *Arinin Aratame Cho*, *Okayama Shi Shi*, *Biyoki*, *Tome Cho*, and *Okayama Shi Shi*, as quoted in Taniguchi, *Okayama Hansei Shi no Kenkyu*, pp. 465-66.

6. The Mino figures are obviously inaccurate for some years, but they have been left unadjusted because it is not yet known just which years are inaccurate or how inaccurate they really are.

7. An index has been added to the figures for comparative purposes. Growth rates were also computed for the district population, but they must be used cautiously. They are less revealing than the index, partly because of the leveling off of the growth in the inner counties, and partly because of the long time periods between years for which statistics exist. Inaccuracies such as the one in the Mino statistics create distorted growth rates. For the period from 1679-1834, Mino appears to have grown by an average of .6 per cent per year, Kojima by .3 per cent, Kamimichi by .2 per cent; the remaining districts showed no annual average growth. The average for Bizen as a whole was .2 per cent per year.

   Growth rates become more significant when the 155-year period is divided into two parts. In the first half of the period, Kamimichi and Tsutaka grew by .7 per cent per year, Kojima and Akasaka by .5 per cent, and Oku, Wake, and Iwanashi by .4 per cent. The distorted Mino rates are at a high 2.3 per cent. The average for Bizen is .5 per cent. The second half of the period witnessed very different growth rates. These were, in fact, zero or negative for all of the districts except Kojima, which had a .2 per cent annual average rate of increase, and Kamimichi, which had .1 per cent. The average for the province was a decrease of .2 per cent per year. On the whole, there was very little growth in Oku, Wake, and Tsutaka, while there seems to have been a slight decrease in numbers in Iwanashi and Akasaka during the 155-year period.

8. Percentages of population were calculated from the figures in Table 2. The land figures are those quoted by Sumio Taniguchi, *Okayama Hansei Shi no Kenkyu*, p. 330. Since the major reclamation projects were carried out in the seventeenth century, the comparison between population and land is more valid than it would otherwise be when data on land are available for only one year.

9. Oku seems to remain the exception to many of the trends in Bizen. Among

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the districts in which extensive land reclamation was carried on, only Oku failed to show an increase in population in the second half of the Tokugawa period. Other factors not yet known must account for the difference between Oku and the seemingly similar districts which sustained population increases.

10. This figure was calculated by aggregating the figures of the provinces of Suo, Nagato, Osumi, Satsuma, Tosa, and Hizen, and thus is weighted by the size of the population in each province. Figures are from Naotaro Sekiyama, *Kinsei Nihon no Jinko Kozo* (The Population Structure of Tokugawa Japan; Tokyo, 1958), pp. 137-39.

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ANTICIPATIONS

GERALD HOLTON

Mach, Einstein, and the Search for Reality

IN THE history of ideas of our century, there is a chapter that might be entitled "The Philosophical Pilgrimage of Albert Einstein," a pilgrimage from a philosophy of science in which sensationism and empiricism were at the center, to one in which the basis was a rational realism. This essay, a portion of a more extensive study,[1] is concerned with Einstein's gradual philosophical reorientation, particularly as it has become discernible during the work on his largely unpublished scientific correspondence.[2]

The earliest known letter by Einstein takes us right into the middle of the case. It is dated 19 March 1901 and addressed to Wilhelm Ostwald.[3] The immediate cause for Einstein's letter was his failure to receive an assistantship at the school where he had recently finished his formal studies, the Polytechnic Institute in Zürich; he now turned to Ostwald to ask for a position at his laboratory, partly in the hope of receiving "the opportunity for further education." Einstein included a copy of his first publication, "Folgerungen aus den Capillaritätserscheinungen" (*Annalen d. Physik*, Vol. 4 [1901], p. 513), which he said had been inspired *(angeregt)* by Ostwald's work; indeed, Ostwald's *Allgemeine Chemie* is the first book mentioned in all of Einstein's published work.

Not having received an answer, Einstein wrote again to Ostwald on 3 April 1901. On 13 April 1901 his father, Hermann Einstein, sent Ostwald a moving appeal, evidently without his son's knowledge. Hermann Einstein reported that his son esteems

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Ostwald "most highly among all scholars currently active in physics."[4]

The choice of Ostwald was significant. He was, of course, not only one of the foremost chemists, but also an active "philosopher-scientist" during the 1890's and 1900's, a time of turmoil in the physical sciences as well as in the philosophy of science. The opponents of kinetic, mechanical, or materialistic views of natural phenomena were vociferous. They objected to atomic theory and gained great strength from the victories of thermodynamics, a field in which no knowledge or assumption was needed concerning the detailed nature of material substances (for example, for an understanding of heat engines).

Ostwald was a major critic of the mechanical interpretation of physical phenomena, as were Helm, Stallo, and Mach. Their form of positivism—as against the sophisticated logical positivism developed later in Carnap and Ayer's work—provided an epistemology for the new phenomenologically based science of correlated observations, linking energetics and sensationism. In the second (1893) edition of his influential textbook on chemistry, Ostwald had given up the mechanical treatment of his first edition for Helm's "energetic" one. "Hypothetical" quantities such as atomic entities were to be omitted; instead, these authors claimed they were satisfied, as Merz wrote around 1904, with "measuring such quantities as are presented directly in observation, such as energy, mass, pressure, volume, temperature, heat, electrical potential, etc., without reducing them to imaginary mechanisms or kinetic quantities." They condemned such conceptions as the ether, with properties not accessible to direct observation, and they issued a call "to consider anew the ultimate principles of all physical reasoning, notably the scope and validity of the Newtonian laws of motion and of the conceptions of force and action, of absolute and relative motion."[5]

All these iconoclastic demands—except anti-atomism—must have been congenial to the young Einstein who, according to his colleague Joseph Sauter, was fond of calling himself "a heretic."[6] Thus, we may well suspect that Einstein felt sympathetic to Ostwald who denied in the *Allgemeine Chemie*[7] that "the assumption of that medium, the ether, is unavoidable. To me it does not seem to be so. . . . There is no need to inquire for a carrier of it when we find it anywhere. This enables us to look upon radiant energy as independently existing in space." It is a position quite consistent

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with that shown later in Einstein's papers of 1905 on photon theory and relativity theory.

In addition, it is worth noting that Einstein, in applying to Ostwald's laboratory, seemed to conceive of himself as an experimentalist. We know from many sources that in his student years in Zürich Einstein's earlier childhood interest in mathematics had slackened considerably. In the *Autobiographical Notes*,[8] Einstein reported: "I really could have gotten a sound mathematical education. However, I worked most of the time in the physical laboratory, fascinated by the direct contact with experience" (p. 15). To this, one of his few reliable biographers adds: "No one could stir him to visit the mathematical seminars. . . . He did not yet see the possibility of seizing that formative power resident in mathematics, which later became the guide of his work. . . . He wanted to proceed quite empirically, to suit his scientific feeling of the time. . . . As a natural scientist, he was a pure empiricist." (Anton Reiser, *Albert Einstein* [New York, 1930], pp. 51-52.)

Ostwald's main philosophical ally was the prolific and versatile Austrian physicist and philosopher Ernst Mach (1838-1916), whose main work Einstein had read avidly in his student years and with whom he was destined to have later the encounters that form a main concern of this paper. Mach's major book, *The Science of Mechanics*,[9] first published in 1883, is perhaps most widely known for its discussion of Newton's *Principia*, in particular for its devastating critique of what Mach called the "conceptual monstrosity of absolute space" (Preface, 7th Edition, 1912)—a conceptual monstrosity because it is "purely a thought-thing which cannot be pointed to in experience." Starting from his analysis of Newtonian presuppositions, Mach proceeded in his announced program of eliminating all metaphysical ideas from science. As Mach said quite bluntly in the preface to the first edition of *The Science of Mechanics:* "This work is not a text to drill theorems of mechanics. Rather, its intention is an enlightening one—or to put it still more plainly, an anti-metaphysical one."

It will be useful to review briefly the essential points of Mach's philosophy. Here we can benefit from a good, although virtually unknown, summary presented by his sympathetic follower, Moritz Schlick, in the essay "Ernst Mach, Der Philosoph."[10]

> Mach was a physicist, physiologist, and also psychologist, and his philosophy . . . arose from the wish to find a principal point of view to which he could hew in any research, one which he would not have to change

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when going from the field of physics to that of physiology or psychology. Such a firm point of view he reached by going back to that which is given before all scientific research: namely, the world of sensations. . . . Since all our testimony concerning the so-called external world rely only on sensations, Mach held that we can and must take these sensations and complexes of sensations to be the sole contents [*Gegenstände*] of those testimonies, and, therefore, that there is no need to assume in addition an unknown reality hidden behind the sensations. With that, the existence *der Dinge an sich* is removed as an unjustified and unnecessary assumption. A body, a physical object, is nothing else than a complex, a more or less firm [we would say, invariant] pattern of sensations, i.e., of colors, sounds, sensations of heat and pressure, etc.

There exists in this world nothing whatever other than sensations and their connections. In place of the word "sensations," Mach liked to use rather the more neutral word "elements." . . . [As is particularly clear in Mach's book *Erkenntnis und Irrtum*,] scientific knowledge of the world consists, according to Mach, in nothing else than the simplest possible description of the connections between the elements, and it has as its only aim the intellectual mastery of those facts by means of the least possible effort of thought. This aim is reached by means of a more and more complete "accommodation of the thoughts to one another." This is the formulation by Mach of his famous "principle of the economy of thought."[11]

The influence of Mach's point of view, particularly in the German-speaking countries, was enormous—on physics, on physiology, on psychology, and on the fields of the history and the philosophy of science[12] (not to mention Mach's profound effect on the young Lenin, Hofmannstal, Musils, among many others outside the sciences). Strangely neglected by recent scholarship—there is not even a major biography—Mach has in the last two or three years again become the subject of a number of promising studies. To be sure, Mach himself always liked to insist that he was beleaguered and neglected, and that he did not have, or wish to have, a philosophical system; yet his philosophical ideas and attitudes had become so widely a part of the intellectual equipment of the period from the 1880's on that Einstein was quite right in saying later that "even those who think of themselves as Mach's opponents hardly know how much of Mach's views they have, as it were, imbibed with their mother's milk."[13]

The problems of physics themselves at that time helped to reinforce the appeal of the new philosophical attitude urged by Mach. The great program of nineteenth-century physics, the reconciliation of the notions of ether, matter, and electricity by means of mechanistic pictures and hypotheses, had led to enormities—for

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example, Larmor's proposal that the electron is a permanent but movable state of twist or strain in the ether, forming discontinuous particles of electricity and possibly of all ponderable matter. To many of the younger physicists of the time, attacking the problems of physics with conceptions inherited from classical nineteenth-century physics did not seem to lead anywhere. And here Mach's iconoclasm and incisive critical courage, if not the details of his philosophy, made a strong impression on his readers.

Mach's Early Influence on Einstein

As the correspondence at the Einstein Archives at Princeton reveals, one of the young scientists deeply caught up in Mach's point of view was Michelange (Michele) Besso—Einstein's oldest and closest friend, fellow student, and colleague at the Patent Office in Bern, the only person to whom Einstein gave public credit for help *(manche wertvolle Anregung)* when he published his basic paper on relativity in 1905. It was Besso who introduced Einstein to Mach's work. In a letter of 8 April 1952 to Carl Seelig, Einstein wrote: "My attention was drawn to Ernst Mach's *Science of Mechanics* by my friend Besso while a student, around the year 1897. The book exerted a deep and persisting impression upon me . . ., owing to its physical orientation toward fundamental concepts and fundamental laws." As Einstein noted in his *Autobiographical Notes*[8] written in 1946, Ernst Mach's *The Science of Mechanics* "shook this dogmatic faith" in "mechanics as the final basis of all physical thinking. . . . This book exercised a profound influence upon me in this regard while I was a student. I see Mach's greatness in his incorruptible skepticism and independence; in my younger years, however, Mach's epistemological position also influenced me very greatly (p. 21)."

As the long correspondence between those old friends shows, Besso remained a loyal Machist to the end. Thus, writing to Einstein on 8 December 1947, he still said: "As far as the history of science is concerned, it appears to me that Mach stands at the center of the development of the last 50 or 70 years." Is it not true, Besso also asked, "that this introduction [to Mach] fell into a phase of development of the young physicist [Einstein] when the Machist style of thinking pointed decisively at observables—perhaps even, indirectly, to clocks and meter sticks?"

Turning now to Einstein's crucial first paper on relativity in

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1905, we can discern in it influences of many, partly contradictory, points of view—not surprising in a work of such originality by a young contributor. Elsewhere I have examined the effect—or lack of effect—on that paper of three contemporary physicists: H. A. Lorentz,[14] Henri Poincaré,[15] and August Föppl.[1] Here we may ask in what sense and to what extent Einstein's initial relativity paper of 1905 was imbued with the style of thinking associated with Ernst Mach and his followers—apart from the characteristics of clarity and independence, the two traits in Mach which Einstein always praised most.

In brief, the answer is that the Machist component—a strong component, even if not the whole story—shows up prominently in two related respects: first, by Einstein's insistence from the beginning of his relativity paper that the fundamental problems of physics cannot be understood until an epistemological analysis is carried out, particularly so with respect to the meaning of the conceptions of space and time[16]; and second, by Einstein's identification of reality with what is given by sensations, the "events," rather than putting reality on a plane beyond or behind sense experience.

From the outset, the instrumentalist, and hence sensationist, views of measurement and of the concepts of space and time are strikingly evident. The key concept in the early part of the 1905 paper is introduced at the top of the third page in a straightforward way. Indeed, Leopold Infeld in his biography of Einstein called them "the simplest sentence[s] I have ever encountered in a scientific paper." Einstein wrote: "We have to take into account that all our judgments in which time plays a part are always judgments of *simultaneous events*. If for instance I say, 'that train arrived here at seven o'clock,' I mean something like this: 'The pointing of the small hand of my watch to seven and the arrival of the train are simultaneous events.' "[17]

The basic concept introduced here, one that overlaps almost entirely Mach's basic "elements," is Einstein's concept of *events (Ereignisse)*—a word that recurs in Einstein's paper about a dozen times immediately following this citation. Transposed into Minkowski's later formulation of relativity, Einstein's "events" are the intersections of particular "word lines," say that of the train and that of the clock. The time (t coordinate) of an event by itself has no operational meaning. As Einstein says: "The 'time' of an event is that which is given simultaneously with the event by a stationary

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clock located at the place of the event" (p. 894). We can say that just as the *time* of an event assumes meaning only when it connects with our consciousness through sense experience (that is, when it is subjected to measurement-in-principle by means of a clock present at the same place), so also is the *place*, or space coordinate, of an event meaningful only if it enters our sensory experience while being subjected to measurement-in-principle (that is, by means of meter sticks present on that occasion at the same time).[18]

This was the kind of operationalist message which, for most of his readers, overshadowed all other philosophical aspects in Einstein's paper. His work was enthusiastically embraced by the groups who saw themselves as philosophical heirs of Mach, the Vienna Circle of neopositivists and its predecessors and related followers,[19] providing a tremendous boost for the philosophy that had initially helped to nurture it. A typical response welcoming the relativity theory as "the victory over the metaphysics of absolutes in the conceptions of space and time . . . a mighty impulse for the development of the philosophical point of view of our time," was extended by J. Petzoldt in the inaugural session of the *Gesellschaft für Positivistische Philosophie* in Berlin, 11 November 1912.[20] Michele Besso, who had heard the message from Einstein before anyone else, had exclaimed: "In the setting of Minkowski's space-time framework, it was now first possible to carry through the thought which the great mathematician, Bernhard Riemann, had grasped: 'The space-time framework itself is formed by the events in it.' "[21]

To be sure, re-reading Einstein's paper with the wisdom of hindsight, as we shall do presently, we can find in it also very different trends, warning of the possibility that "reality" in the end is not going to be left identical with "events." There are premonitions that sensory experiences, in Einstein's later work, will not be regarded as the chief building blocks of the "world," that the laws of physics themselves will be seen to be built into the event-world as the undergirding structure "governing" the pattern of events.

Such precursors appear even earlier, in one of Einstein's early letters in the Archives. Addressed to his friend Marcel Grossmann, it is dated 14 April 1901, when Einstein believed he had found a connection between Newtonian forces and the forces of attraction between molecules: "It is a wonderful feeling to recognize the unity of a complex of appearances which, to direct sense experience, seem to be separate things." Already there is a hint here of the

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high value that will be placed on intuited unity and the limited role seen for evident sense experience.

But all this was not yet ready to come into full view, even to the author. Taking the early papers as a whole, and in the context of the physics of the day, we find that Einstein's philosophical pilgrimage did start on the historic ground of positivism. Moreover, Einstein thought so himself, and confessed as much in letters to Ernst Mach.

The Einstein-Mach Letters

In the history of recent science, the relation between Einstein and Mach is an important topic that has begun to interest a number of scholars. Indeed, it is a drama of which we can sketch here four stages: Einstein's early acceptance of the main features of Mach's doctrine; the Einstein-Mach correspondence and meeting; the revelation in 1921 of Mach's unexpected and vigorous attack on Einstein's relativity theory; and Einstein's own further development of a philosophy of knowledge in which he rejected many, if not all, of his earlier Machist beliefs.

Happily, the correspondence is preserved at least in part. A few letters have been found, all from Einstein to Mach. Those of concern here are part of an exchange between 1909 and 1913, and they testify to Einstein's deeply felt attraction to Mach's viewpoint, just at a time when the mighty Mach himself—forty years senior to the young Einstein whose work was just becoming widely known—had for his part embraced the relativity theory publicly by writing in the second (1909) edition of *Conservation of Energy:* "I subscribe, then, to the principle of relativity, which is also firmly upheld in my *Mechanics* and *Wärmelehre.*"[22] In the first letter, Einstein writes from Berne on 9 August 1909. Having thanked Mach for sending him the book on the law of conservation of energy, he adds: "I know, of course, your main publications very well, of which I most admire your book on Mechanics. You have had such a strong influence upon the epistemological conceptions of the younger generation of physicists that even your opponents today, such as Planck, undoubtedly would have been called Mach followers by physicists of the kind that was typical a few decades ago."

It will be important for our analysis to remember that Planck was Einstein's earliest patron in scientific circles. It was Planck who, in 1905, as editor of the *Annalen der Physik,* received Einstein's first relativity paper and thereupon held a review seminar on the

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paper in Berlin. Planck defended Einstein's work on relativity in public meetings from the beginning, and by 1913 had succeeded in persuading his German colleagues to invite Einstein to the Kaiser-Wilhelm-Gesellschaft in Berlin. With a polemical essay "Against the New Energetics" in 1896, he had made clear his position, and by 1909 Planck was one of the few opponents of Mach, and scientifically the most prominent one. He had just written a famous attack, *Die Einheit des physikalischen Weltbildes*. Far from accepting Mach's view that, as he put it, "Nothing is real except the perceptions, and all natural science is ultimately an economic adaptation of our ideas to our perceptions," Planck held to the entirely antithetical position that a basic aim of science is "the finding of a *fixed* world picture independent of the variation of time and people," or, more generally, "the complete liberation of the physical picture from the individuality of the separate intellects."[23] At least by implication in Einstein's remarks to Mach, he dissociated himself from allegiance to Planck's view. It may also not be irrelevant that just at that time Einstein, who since 1906 had been objecting to inconsistencies in Planck's quantum theory, was preparing his first major invited paper before a scientific congress, the eighty-first meeting of the Naturforscherversammlung, announced for September, 1909, in Salzburg. Einstein's paper called for a revision of Maxwell's theory to accommodate the probabilistic character of the emission of photons—none of which Planck could accept—and concluded: "To accept Planck's theory means, in my view, to throw out the bases of our [1905] theory of radiation."

Mach's reply to Einstein's first letter is now lost, but it must have come quickly, because eight days later Einstein sends an acknowledgment:

> Berne, 17 August 1909. Your friendly letter gave me enormous pleasure. . . . I am very glad that you are pleased with the relativity theory. . . . Thanking you again for your friendly letter, I remain, your student [indeed: *Ihr Sie verehrender Schüler*], A. Einstein.

Einstein's next letter was written as physics professor in Prague, where Mach before him had been for twenty-eight years. The post had been offered to Einstein on the basis of recommendations of a faction (Lampa, Pick) who regarded themselves as faithful disciples of Mach. The letter was sent out about New Year's 1911-12, perhaps just before or after Einstein's sole (and, according to P. Frank's account in *Einstein, His Life and Times*, not very

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successful) visit to Mach, and after the first progress toward the general relativity theory:

> . . . I can't quite understand how Planck has so little understanding for your efforts. His stand to my [general relativity] theory is also one of refusal. But I can't take it amiss; so far, that one single epistemological argument is the only thing which I can bring forward in favor of my theory.

Here, Einstein is referring delicately to the Mach Principle, which he had been putting at the center of the developing theory.[24] Mach responded by sending Einstein a copy of one of his books, probably the *Analysis of Sensations.*

In the last of these letters to Mach (who was now seventy-five years old, and for some years had been paralyzed), Einstein writes from Zürich on 25 June 1913:

> Recently you have probably received my new publication on Relativity and Gravitation which I have at last finished after unending labor and painful doubt. [This must have been the "Entwurf einer verallgemeinerten Relativitätstheorie und einer Theorie der Gravitation," written with Marcel Grossmann.[25]] Next year at the solar eclipse it will turn out whether the light rays are bent by the sun, in other words whether the basic and fundamental assumption of the equivalence of the acceleration of the reference frame and of the gravitational field really holds. If so, then your inspired investigations into the foundations of mechanics—despite Planck's unjust criticism—will receive a splendid confirmation. For it is a necessary consequence that inertia has its origin in a kind of mutual interaction of bodies, fully in the sense of your critique of Newton's bucket experiment.[26]

The Paths Diverge

The significant correspondence stops here, but Einstein's public and private avowals of his adherence to Mach's ideas continue for several years more. For example, there is his well-known, moving eulogy of Mach, published in 1916.[13] In August, 1918, Einstein writes to Besso quite sternly about an apparent—and quite temporary—lapse in Besso's positivistic epistemology; it is an interesting letter, worth citing in full:

> 28 August 1918.
> Dear Michele:
> In your last letter I find, on re-reading, something which makes me angry: That speculation has proved itself to be superior to empiricism. You are thinking here about the development of relativity theory. However, I find that this development teaches something else, that it is prac-

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tically the opposite, namely that a theory which wishes to deserve trust must be built upon generalizable facts.

Old examples: Chief postulates of thermodynamics [based] on impossibility of perpetuum mobile. Mechanics [based] on grasped [*ertasteten*] law of inertia. Kinetic gas theory [based] on equivalence of heat and mechanical energy (also historically). Special Relativity on the constancy of light velocity and Maxwell's equation for the vacuum, which in turn rest on empirical foundations. Relativity with respect to uniform [?] translation is a *fact of experience.*

General Reality: *Equivalence of inertial and gravitational mass.* Never has a truly useful and deep-going theory really been found purely speculatively. The nearest case is Maxwell's hypothesis concerning displacement current; there the problem was to do justice to the fact of light propagation. . . . With cordial greetings, your Albert. [Emphasis in the original]

Careful reading of this letter shows us that already here there is evidence of divergence between the conception of "fact" as understood by Einstein and "fact" as understood by a true Machist. The impossibility of the *perpetuum mobile,* the first law of Newton, the constancy of light velocity, the validity of Maxwell's equations, the equivalence of inertial and gravitational mass—none of these would have been called "facts of experience" by Mach. Indeed, Mach might have insisted that—to use one of his favorite battle words—it is evidence of "dogmatism" not to regard all these conceptual constructs as continually in need of probing re-examination; thus, Mach had written:[27]

. . . for me, matter, time and space are still *problems,* to which, incidentally, the physicists (Lorentz, Einstein, Minkowski) are also slowly approaching.

Similar evidence of Einstein's gradual apostasy appears in a letter of 4 December 1919 to Paul Ehrenfest. Einstein writes:

I understand your difficulties with the development of relativity theory. They arise simply because you want to base the innovations of 1905 on epistemological grounds (nonexistence of the stagnant ether) instead of empirical grounds (equivalence of all inertial systems with respect to light).

Mach would have applauded Einstein's life-long suspicion of formal epistemological systems, but how strange would he have found this use of the word *empirical* to characterize the hypothesis of the equivalence of all inertial systems with respect to light! What we see forming slowly here is Einstein's view that the fundamental

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role played by experience in the construction of fundamental physical theory is, after all, not through the "atom" of experience, not through the individual sensation or the protocol sentence, but through some creative digest or synthesis of *"die gesammten Erfahrungstatsachen,"* the *totality* of physical experience.[28] But all this was still hidden. Until Mach's death, and for several years after, Einstein considered and declared himself a disciple of Mach.

In the meantime, however, unknown to Einstein and everyone else, a time bomb had been ticking away. Set in 1913, it went off in 1921, five years after Mach's death, when Mach's *The Principles of Physical Optics* was published at last. Mach's preface was dated July, 1913—perhaps a few days or, at most, a few weeks after Mach had received Einstein's last, enthusiastic letter and the article on general relativity theory. In a well-known passage in the preface (but one usually found in an inaccurate translation), Mach had written:

> I am compelled, in what may be my last opportunity, to cancel my views [*Anschauungen*] of the relativity theory.
>
> I gather from the publications which have reached me, and especially from my correspondence, that I am gradually becoming regarded as the forerunner of relativity. I am able even now to picture approximately what new expositions and interpretations many of the ideas expressed in my book on Mechanics will receive in the future from this point of view. It was to be expected that philosophers and physicists should carry on a crusade against me, for, as I have repeatedly observed, I was merely an unprejudiced rambler endowed with original ideas, in varied fields of knowledge. I must, however, as assuredly disclaim to be a forerunner of the relativists as I personally reject the atomistic doctrine of the present-day school, or church. The reason why, and the extent to which, I reject [*ablehne*] the present-day relativity theory, which I find to be growing more and more dogmatical, together with the particular reasons which have led me to such a view—considerations based on the physiology of the senses, epistemological doubts, and above all the insight resulting from my experiments—must remain to be treated in the sequel [a sequel which was never published].

Certainly, Einstein was deeply disappointed by this belated disclosure of Mach's sudden dismissal of the relativity theory. Some months later, during a lecture on 6 April 1922 in Paris, in a discussion with the anti-Machist philosopher Emile Meyerson, Einstein allowed in a widely reported remark that Mach was *"un bon méchanicien,"* but a *"deplorable philosophe."*[29]

We can well understand that Mach's rejection was at heart very

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painful, the more so as it was somehow Einstein's tragic fate to have the contribution he most cared about rejected by the very men whose approval and understanding he would have most gladly had —a situation not unknown in the history of science. In addition to Mach, the list includes these four: *H. Poincaré,* who, to his death in 1912, only once deigned to mention Einstein's name in print, and then only to register an objection; *H. A. Lorentz,* who gave Einstein personally every possible encouragement—short of fully accepting the theory of relativity for himself; *Planck,* whose support of the special theory of relativity was unstinting, but who resisted Einstein's ideas on general relativity and the early quantum theory of radiation; and *A. A. Michelson,* who to the end of his days did not believe in relativity theory, and once said to Einstein that he was sorry that his own work may have helped to start this "monster."[30]

Soon Einstein's generosity again took the upper hand and resulted, from then to the end of his life, in many further personal testimonies to Mach's earlier influence.[31] A detailed analysis was provided in Einstein's letter of 8 January 1948 to Besso:

> As far as Mach is concerned, I wish to differentiate between Mach's influence in general and his influence on me. . . . Particularly in the *Mechanics* and the *Wärmelehre* he tried to show how conceptions arose out of experience. He took convincingly the position that these conceptions, even the most fundamental ones, obtained their warrant only out of empirical knowledge, that they are in no way logically necessary. . . .
>
> I see his weakness in this, that he more or less believed science to consist in a mere ordering of empirical material; that is to say, he did not recognize the freely constructive element in formation of concepts. In a way he thought that theories arise through *discoveries* and not through *inventions.* He even went so far that he regarded "sensations" not only as material which has to be investigated, but, as it were, as the building blocks of the real world; thereby, he believed, he could overcome the difference between psychology and physics. If he had drawn the full consequences, he would have had to reject not only atomism but also the idea of a physical reality.
>
> Now, as far as Mach's influence on my own development is concerned, it certainly was great. I remember very well that you drew my attention to his *Mechanics* and *Wärmelehre* during my first years of study, and that both books made a great impression on me. The extent to which they influenced my own work is, to say the truth, not clear to me. As far as I am conscious of it, the immediate influence of Hume on me was greater. . . . But, as I said, I am not able to analyze that which lies anchored in unconscious thought. It is interesting, by the way, that Mach rejected the special relativity theory passionately (he did not live to see the general relativity theory [in the developed form]). The theory

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was, for him, inadmissibly speculative. He did not know that this speculative character belongs also to Newton's mechanics, and to every theory which thought is capable of. There exists only a gradual difference between theories, insofar as the chains of thought from fundamental concepts to empirically verifiable conclusions are of different lengths and complications.[32]

Antipositivistic Component of Einstein's Work

Ernst Mach's harsh words in his 1913 preface leave a tantalizing mystery. Ludwig Mach's destruction of his father's papers has so far made it impossible to find out more about the "experiments" (possibly on the constancy of the velocity of light) at which Ernst Mach hinted. Since 1921, many speculations have been offered to explain Mach's remarks.[33] They all leave something to be desired. Yet, I believe, it is not so difficult to reconstruct the main reasons why Mach ended up rejecting the relativity theory. To put it very simply, Mach had recognized more and more clearly, years before Einstein did so himself, that Einstein had indeed fallen away from the faith, had left behind him the confines of Machist empiriocriticism.

The list of evidences is long. Here only a few examples can be given, the first from the 1905 relativity paper itself: What had made it really work was that it contained and combined elements based on two entirely different philosophies of science—not merely the empiricist-operationist component, but the courageous initial postulation, in the second paragraph, of two thematic hypotheses (one on the constancy of light velocity and the other on the extension of the principle of relativity to all branches of physics), two postulates for which there was and can be no direct empirical confirmation.

For a long time, Einstein did not draw attention to this feature. In a lecture at King's College, London, in 1921, just before the posthumous publication of Mach's attack, Einstein still was protesting that the origin of relativity theory lay in the facts of direct experience:

> . . . I am anxious to draw attention to the fact that this theory is not speculative in origin; it owes its invention entirely to the desire to make physical theory fit observed fact as well as possible. We have here no revolutionary act, but the natural continuation of a line that can be traced through centuries. The abandonment of certain notions connected with space, time, and motion, hitherto treated as fundamentals, must not be regarded as arbitrary, but only as conditioned by observed facts.[34]

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By June, 1933, however, when Einstein returned to England to give the Herbert Spencer Lecture at Oxford entitled "On the Method of Theoretical Physics," the more complex epistemology that was in fact inherent in his work from the beginning had begun to be expressed. He opened this lecture with the significant sentence: "If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: Don't listen to their words, fix your attention on their deeds." He went on to divide the tasks of experience and reason in a very different way from that advocated in his earlier visit to England:

> We are concerned with the eternal antithesis between the two inseparable components of our knowledge, the empirical and the rational. . . . The structure of the system is the work of reason; the empirical contents and their mutual relations must find their representation in the conclusions of the theory. In the possibility of such a representation lie the sole value and justification of the whole system, and especially the concepts and fundamental principles which underlie it. Apart from that, these latter are free inventions of the human intellect, which cannot be justified either by the nature of that intellect or in any other fashion *a priori*.

In the summary of this section, he draws attention to the "purely fictitious character of the fundamentals of scientific theory." It is this penetrating insight which Mach must have smelled out much earlier and dismissed as "dogmatism."

Indeed, Einstein, in his 1933 Spencer Lecture—widely read, as were and still are so many of his essays—castigates the old view that "the fundamental concepts and postulates of physics were not in the logical sense inventions of the human mind but could be deduced from experience by 'abstraction'—that is to say, by logical means. A clear recognition of the erroneousness of this notion really only came with the general theory of relativity."

Einstein ends this discussion with the enunciation of his current credo, so far from that he had expressed earlier:

> Nature is the realization of the simplest conceivable mathematical ideas. I am convinced that we can discover, by means of purely mathematical constructions, those concepts and those lawful connections between them which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of physical utility of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed.[85]

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Technically, Einstein was now at—or rather just past—the midstage of his pilgrimage. He had long ago abandoned his youthful allegiance to a primitive phenomenalism that Mach would have commended. In the first of the two passages just cited and others like it, he had gone on to a more refined form of phenomenalism which many of the logical positivists could still accept. He has, however, gone beyond it in the second passage, turning toward interests that we shall see later to have matured into clearly metaphysical conceptions.

Later, Einstein himself stressed the key role of what we have called thematic rather than phenomenic elements[36]—and thereby he fixed the early date at which, in retrospect, he found this need to arise in his earliest work. Thus he wrote in his *Autobiographical Notes* of 1946 that "shortly after 1900 . . . I despaired of the possibility of discovering the true laws by means of constructive efforts based on known facts. The longer and the more despairingly I tried, the more I came to the conviction that only the discovery of *a universal formal principle* could lead us to assured results."[37]

Another example of evidence of the undercurrent of disengagement from a Machist position is an early one: It comes from Einstein's article on relativity in the 1907 *Jahrbuch der Radioaktivität und Elektronik* (Vol. 4, No. 4), where Einstein responds, after a year's silence, to W. Kaufmann's paper in the *Annalen der Physik* (Vol. 19, 1906). That paper had been the first publication in the *Annalen* to mention Einstein's work on the relativity theory, published there the previous year. Coming from the eminent experimental physicist Kaufmann, it had been most significant that this very first discussion was announced as a categorical, experimental disproof of Einstein's theory. Kaufmann had begun his attack with the devastating summary:

> I anticipate right here the general result of the measurements to be described in the following: *the measurement results are not compatible with the Lorentz-Einsteinian fundamental assumption.*[38]

Einstein could not have known that Kaufmann's equipment was inadequate. Indeed, it took ten years for this to be fully realized, through the work of Guye and Lavanchy in 1916. So in his discussion of 1907, Einstein had to acknowledge that there seemed to be small but significant differences between Kaufmann's results and Einstein's predictions. He agreed that Kaufmann's calculations seemed to be free of error, but "whether there is an unsuspected

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systematic error or whether the foundations of relativity theory do not correspond with the facts one will be able to decide with certainty only if a great variety of observational material is at hand."

Despite this prophetic remark, Einstein does not rest his case on it. On the contrary, he has a very different, and what for his time and situation must have been a very daring, point to make: He acknowledges that the theories of electron motion given earlier by Abraham and by Bucherer do give predictions considerably closer to the experimental results of Kaufmann. But Einstein refuses to let the "facts" decide the matter: "In my opinion both theories have a rather small probability, because their fundamental assumptions concerning the mass of moving electrons are not explainable in terms of theoretical systems which embrace a greater complex of phenomena."[39]

This is the characteristic position—the crucial difference between Einstein and those who make the correspondence with experimental fact the chief deciding factor for or against a theory: Even though the "experimental facts" at that time very clearly seemed to favor the theory of his opponents rather than his own, he finds the *ad hoc* character of their theories more significant and objectionable than an apparent disagreement between his theory and their "facts."[40]

So already in this 1907 article—which, incidentally, Einstein mentions in his postcard of 17 August 1909 to Ernst Mach, with a remark regretting that he has no more reprints for distribution—we have explicit evidence of a hardening of Einstein against the epistemological priority of experiment, not to speak of sensory experience. In the years that followed, Einstein more and more openly put the consistency of a simple and convincing theory or of a thematic conception higher in importance than the latest news from the laboratory—and again and again he turned out to be right.

Thus, only a few months after Einstein had written in his fourth letter to Mach that the solar eclipse experiment will decide "whether the basic and fundamental assumption of the equivalence of the acceleration of the reference frame and of the gravitational field really holds," Einstein writes to Besso in a very different vein (in March 1914), before the first, ill-fated eclipse expedition was scheduled to test the conclusions of the preliminary version of the general relativity theory: "Now I am fully satisfied, and I do not doubt any more the correctness of the whole system, may the observation of the eclipse succeed or not. The sense of the thing [*die*

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Vernunft der Sache] is too evident." And later, commenting on the fact that there remains up to 10 per cent discrepancy between the measured deviation of light owing to the sun's field and the calculated effect based on the general relativity theory: "For the expert, this thing is not particularly important, because the main significance of the theory does not lie in the verification of little effects, but rather in the great simplification of the theoretical basis of physics as a whole."[41] Or again, in Einstein's "Notes on the Origin of the General Theory of Relativity,"[42] he reports that he "was in the highest degree amazed" by the existence of the equivalence between inertial and gravitational mass, but that he "had no serious doubts about its strict validity, even without knowing the results of the admirable experiment of Eötvös."

The same point is made again in a revealing account given by Einstein's student, Ilse Rosenthal-Schneider. In a manuscript "Reminiscences of Conversation with Einstein," dated 23 July 1957, she reports:

> Once when I was with Einstein in order to read with him a work that contained many objections against his theory . . . he suddenly interrupted the discussion of the book, reached for a telegram that was lying on the windowsill, and handed it to me with the words, "Here, this will perhaps interest you." It was Eddington's cable with the results of measurement of the eclipse expedition [1919]. When I was giving expression to my joy that the results coincided with his calculations, he said quite unmoved, "But I knew that the theory is correct"; and when I asked, what if there had been no confirmation of his prediction, he countered: "Then I would have been sorry for the dear Lord—the theory *is* correct."[43]

Minkowski's "World" and the World of Sensations

The third major point at which Mach, if not Einstein himself, must have seen that their paths were diverging is the development of relativity theory into the geometry of the four-dimensional space-time continuum, begun in 1907 by the mathematician H. Minkowski (who, incidentally, had had Einstein as a student in Zürich). Indeed, it was through Minkowski's semipopular lecture, "Space and Time," on 21 September 1908 at the eightieth meeting of the Naturforscherversammlung,[44] that a number of scientists first became intrigued with relativity theory. We have several indications that Mach, too, was both interested in and concerned about the introduction of four-dimensional geometry into physics (in Mach's cor-

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respondence around 1910, for example, with A. Föppl); according to F. Herneck,[45] Ernst Mach specially invited the young Viennese physicist Philipp Frank to visit him "in order to find out more about the relativity theory, above all about the use of four-dimensional geometry." As a result, Frank, who had recently finished his studies under Ludwig Boltzmann and had begun to publish noteworthy contributions to relativity, published the "presentation of Einstein's theory to which Mach gave his assent" under the title "Das Relativitätsprinzip und die Darstellung der physikalischen Erscheinungen im vierdimensionalen Raum."[46] It is an attempt, addressed to readers "who do not master modern mathematical methods," to show that Minkowski's work brings out the "empirical facts far more clearly by the use of four-dimensional world lines." The essay ends with the reassuring conclusion: "In this four-dimensional world the facts of experience can be presented more adequately than in three-dimensional space, where always only an arbitrary and one-sided projection is pictured."

Following Minkowski's own papers on the whole, Frank's treatment can make it nevertheless still appear that in most respects the time dimension is equivalent to the space dimensions. Thereby one could think that Minkowski's treatment based itself not only on a functional and operational interconnection of space and time, but also—fully in accord with Mach's own views—on the primacy of ordinary, "experienced" space and time in the relativistic description of phenomena.

Perhaps as a result of this presentation, Mach invoked the names of Lorentz, Einstein, and Minkowski in his reply of 1910 to Planck's first attack, citing them as physicists "who are moving closer to the problems of matter, space, and time." Already a year earlier, Mach seems to have been hospitable to Minkowski's presentation, although not without reservations. Mach wrote in the 1909 edition of *Conservation of Energy*[22]: "Space and time are here conceived not as independent entities, but as forms of the dependence of the phenomena on one another"; he also added a reference to Minkowski's lecture of 1908.[44] But a few lines earlier, Mach had written: "Spaces of many dimensions seem to me not so essential for physics. I would only uphold them if things of thought [*Gedankendinge*] like atoms are maintained to be indispensable, and if, then, also the freedom of working hypotheses is upheld."

It was correctly pointed out by C. B. Weinberg[47] that Mach may eventually have had two sources of suspicion against the Minkow-

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skian form of relativity theory. As was noted above, Mach regarded the fundamental notions of mechanics as problems to be continually discussed with maximum openness within the frame of empiricism, rather than as questions that can be solved and settled —as the relativists, seemingly dogmatic and sure of themselves, were in his opinion more and more inclined to do. In addition, Mach held that the questions of physics were to be studied in a broader setting, encompassing biology and psychophysiology. Thus Mach wrote: "Physics is not the entire world; biology is there too, and belongs essentially to the world picture."[48]

But I see also a third reason for Mach's eventual antagonism against such conceptions as Minkowski's (unless one restricted their application to "mere things of thought like atoms and molecules, which by their very nature can never be made the objects of sensuous contemplations"[49]). If one takes Minkowski's essay seriously—for example, the abandonment of space and time separately, with identity granted only to "a kind of union of the two"—one must recognize that it entails the abandonment of the conceptions of experiential space and experiential time; and that is an attack on the very roots of sensations-physics, on the meaning of actual measurements. If identity, meaning, or "reality" lies in the four-dimensional space-time interval *ds,* one is dealing with a quantity which is hardly *denkökonomisch,* nor one that preserves the primacy of measurements in "real" space and time. Mach may well have seen the warning flag; and worse was soon to come, as we shall see at once.

In his exuberant lecture of 1908 (see Ref. 44), Minkowski had announced that "three-dimensional geometry becomes a chapter in four-dimensional physics. . . . Space and time are to fade away into the shadows, and only *eine Welt an sich* will subsist." In this "world" the crucial innovation is the conception of the "*zeitartige Vektorelement,*" *ds,* defined as $(1/c)\sqrt{c^2dt^2-dx^2-dy^2-dz^2}$ with imaginary components. To Mach, the word *Element* had a crucial and very different meaning. As we saw in Schlick's summary, elements were nothing less than the sensations and complexes of sensations of which the world consists and which completely define the world. Minkowski's rendition of relativity theory was now revealing the need to move the ground of basic, elemental truths from the plane of direct experience in ordinary space and time to a mathematicized, formalistic model of the world in a union of space and time that is not directly accessible to sensation—and, in this respect, is

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reminiscent of absolute space and time concepts that Mach had called "metaphysical monsters."[50]

Here, then, is an issue which, from the beginning, had separated Einstein and Mach even before they realized it. To the latter, the fundamental task of science was economic and descriptive; to the former, it was speculative-constructive and intuitive. Mach had once written: "If all the individual facts—all the individual phenomena, knowledge of which we desire—were immediately accessible to us, science would never have arisen."[51] To this, with the forthrightness caused perhaps by his recent discovery of Mach's opposition, Einstein countered during his lecture in Paris of 6 April 1922: "Mach's system studies the existing relations between data of experience: for Mach, science is the totality of these relations. That point of view is wrong, and in fact what Mach has done is to make a catalog, not a system."[52]

We are witnessing here an old conflict, one that has continued throughout the development of the sciences. Mach's phenomenalism brandished an undeniable and irresistible weapon for the critical re-evaluation of classical physics, and in this it seems to hark back to an ancient position that looked upon sensuous appearances as the beginning and end of scientific achievement. One can read Galileo in this light, when he urges the primary need of *description* for the fall of bodies, leaving "the causes" to be found out later. So one can understand (or rather, misunderstand) Newton, with his too-well-remembered remark: "I feign no hypotheses."[53] Kirchhoff is in this tradition. Boltzmann wrote of him in 1888:

> The aim is not to produce bold hypotheses as to the essence of matter, or to explain the movement of a body from that of molecules, but to present equations which, free from hypotheses, are as far as possible true and quantitatively correct correspondents of the phenomenal world, careless of the essence of things and forces. In his book on mechanics, Kirchhoff will ban all metaphysical concepts, such as forces, the cause of a motion; he seeks only the equations which correspond so far as possible to observed motions.[54]

And so could, and did, Einstein himself understand the Machist component of his own early work.

Phenomenalistic positivism in science has always been victorious, but only up to a very definite limit. It is the necessary sword for destroying old error, but it makes an inadequate plowshare for cultivating a new harvest. I find it exceedingly significant that Einstein saw this during the transition phase of partial disengage-

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ment from the Machist philosophy. In the spring of 1917 Einstein wrote to Besso and mentioned a manuscript which Friedrich Adler had sent him. Einstein commented: "He rides Mach's poor horse to exhaustion." To this, Besso—the loyal Machist—responds on 5 May 1917: "As to Mach's little horse, we should not insult it; did it not make possible the infernal journey through the relativities? And who knows—in the case of the nasty quanta, it may also carry Don Quixote de la Einsta through it all!"

Einstein's answer of 13 May 1917 is revealing: "I do not inveigh against Mach's little horse; but you know what I think about it. It cannot give birth to anything living, it can only exterminate harmful vermin."

Toward a Rationalistic Realism

The rest of the pilgrimage is easy to reconstruct, as Einstein more and more openly and consciously turned Mach's doctrine upside down—minimizing rather than maximizing the role of actual details of experience, both at the beginning and at the end of scientific theory, and opting for a rationalism that almost inevitably would lead him to the conception of an objective, "real" world behind the phenomena to which our senses are exposed.

In the essay, "Maxwell's Influence on the Evolution of the Idea of Physical Reality" (1931), Einstein began with a sentence that could have been taken almost verbatim from Max Planck's attack on Mach in 1909, cited above: "The belief in an external world independent of the perceiving subject is the basis of all natural science." Again and again, in the period beginning with his work on the general relativity theory, Einstein insisted that between experience and reason, as well as between the world of sensory perception and the objective world, there are logically unbridgeable chasms. He characterized the efficacy of reason to grasp reality by the word *miraculous;* the very terminology in these statements would have been anathema to Mach.

We may well ask when and under what circumstances Einstein himself became aware of his change. Here again, we may turn for illumination to one of the hitherto unpublished letters, one written to his old friend, C. Lanczos, on 24 January 1938:

> Coming from sceptical empiricism of somewhat the kind of Mach's, I was made, by the problem of gravitation, into a believing rationalist, that is, one who seeks the only trustworthy source of truth in mathematical

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simplicity. The logically simple does not, of course, have to be physically true; but the physically true is logically simple, that is, it has unity at the foundation.

Indeed, all evidence points to the conclusion that Einstein's work on general relativity theory was crucial in his epistemological development. As he wrote later in "Physics and Reality" (1936): "the first aim of the general theory of relativity was the preliminary version which, while not meeting the requirements for constituting a closed system, could be connected in as simple a manner as possible with 'directly observed facts.'" But the aim, still apparent during the first years of correspondence with Mach, could not be achieved. In notes on the origin of the general relativity theory, Einstein reported:

> I soon saw that the inclusion of non-linear transformation, as the principle of equivalence demanded, was inevitably fatal to the simple physical interpretation of the coordinate—i.e., that it could no longer be required that coordinate differences [*ds*] should signify direct results of measurement with ideal scales or clocks. I was much bothered by this piece of knowledge . . . [just as Mach must have been].
>
> The solution of the above mentioned dilemma [from 1912 on] was therefore as follows: A physical significance attaches not to the differentials of the coordinates, but only to the Riemannian metric corresponding to them.[55]

And this is precisely a chief result of the 1913 essay of Einstein and Grossmann,[56] the same paper which Einstein sent to Mach and discussed in his fourth letter. This result was the final consequence of the Minkowskian four-space representation—the sacrifice of the primacy of direct sense perception in constructing a physically significant system. It was the choice that Einstein had to make—against fidelity to a catalogue of individual operational experiences and in favor of fidelity to the ancient hope for a unity at the base of physical theory.[57]

Enough has been written in other places to show the connections that existed between Einstein's scientific rationalism and his religious beliefs. Max Born summarized it in one sentence: "He believed in the power of reason to guess the laws according to which God has built the world."[58] Perhaps the best expression of this position by Einstein himself is to be found in his essay, "Über den gegenwärtigen Stand der Feld-Theorie," in the *Festschrift* of 1929 for Aurel Stodola:[59]

> Physical Theory has two ardent desires, to gather up as far as possible all

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pertinent phenomena and their connections, and to help us not only to know *how* Nature is and *how* her transactions are carried through, but also to reach as far as possible the perhaps utopian and seemingly arrogant aim of knowing why Nature is *thus and not otherwise*. Here lies the highest satisfaction of a scientific person. . . . [On making deductions from a "fundamental hypothesis" such as that of the kinetic-molecular theory,] one experiences, so to speak, that God Himself could not have arranged those connections [between, for example, pressure, volume, and temperature] in any other way than that which factually exists, any more than it would be in His power to make the number 4 into a prime number. This is the promethean element of the scientific experience. . . . Here has always been for me the particular magic of scientific considerations; that is, as it were, the religious basis of scientific effort.

This fervor is indeed far from the kind of analysis which Einstein had made only a few years earlier. It is doubly far from the asceticism of his first philosophic mentor, Mach, who had written in his day book: "Colors, space, tones, etc. These are the only realities. Others do not exist."[60] It is, on the contrary, far closer to the rational realism of his first scientific mentor Planck, who had written: "The disjointed data of experience can never furnish a veritable science without the intelligent interference of a spirit actuated by faith. . . . We have a right to feel secure in surrendering to our belief in a philosophy of the world based upon a faith in the rational ordering of this world."[61] Indeed, we note the philosophical kinship of Einstein's position with seventeenth-century natural philosophers—for example, with Johannes Kepler who, in the preface of the *Mysterium Cosmographicum,* announced that he wanted to find out concerning the number, positions, and motions of the planets, "why they are as they are, and not otherwise," and who wrote to Herwart in April, 1599, that, with regard to numbers and quantity, "our knowledge is of the same kind as God's, at least insofar as we can understand something of it in this mortal life."

Not unexpectedly, we find that during this period (around 1930) Einstein's non-scientific writings began to refer to religious questions much more frequently than before. There is a close relation between his epistemology, in which reality does not need to be validated by the individual's sensorium, and what he called "Cosmic religion,"[62] defined as follows: "The individual feels the vanity of human desires and aims, and the nobility and marvelous order which are revealed in nature and in the world of thought. He feels the individual destiny as an imprisonment and seeks to experience the totality of existence as a unity full of significance."

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Needless to say, Einstein's friends from earlier days sometimes had to be informed of his change of outlook in a blunt way. For example, Einstein wrote to Moritz Schlick on 28 November 1930:

> In general your presentation fails to correspond to my conceptual style insofar as I find your whole orientation so to speak too positivistic. . . . I tell you straight out: Physics is the attempt at the conceptual construction of a model of the *real world* and of its lawful structure. To be sure, it [physics] must present exactly the empirical relations between those sense experiences to which we are open; but only *in this way* is it chained to them. . . . In short, I suffer under the (unsharp) separation of Reality of Experience and Reality of Being. . . .
>
> You will be astonished about the "metaphysicist" Einstein. But every four- and two-legged animal is de facto in this sense metaphysicist. [Emphasis in the original]

Similarly, P. Frank, Einstein's early associate and later his biographer, reports that the realization of Einstein's true state of thought reached Frank in a most embarrassing way, at the congress of German physicists in Prague in 1929, just as Frank was delivering "an address in which I attacked the metaphysical position of the German physicists and defended the positivistic ideas of Mach." The very next speaker disagreed and showed Frank that he had been mistaken still to associate Einstein's views with that of Mach and himself. "He added that Einstein was entirely in accord with Planck's view that physical laws describe a reality in space and time that is independent of ourselves. At that time," Frank comments, "this presentation of Einstein's views took me very much by surprise."[63]

In retrospect it is, of course, much easier to see the evidences that this change was being prepared. Einstein himself realized more and more clearly how closely he had moved to Planck, from whom he earlier dissociated himself in three of the four letters to Mach. At the celebration of Planck's sixtieth birthday, two years after Mach's death, Einstein made a moving speech in which, perhaps for the first time, he referred publicly to the Planck-Mach dispute and affirmed his belief that "there is no logical way to the discovery of these elementary laws. There is only the way of intuition" based on *Einfühlung* in experience.[64] The scientific dispute concerning the theory of radiation between Einstein and Planck, too, had been settled (in Einstein's favor) by a sequence of developments after 1911—for example, by Bohr's theory of radiation from gas atoms. As colleagues, Planck and Einstein saw each other regularly from 1913

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on. Among evidences of the coincidence of these outlooks there is in the Einstein Archives a handwritten draft, written on or just before 17 April 1931 and intended as Einstein's introduction to Planck's hard-hitting article "Positivism and the Real External World."[65] In lauding Planck's article, Einstein concludes: "I presume I may add that both Planck's conception of the logical state of affairs as well as his subjective expectation concerning the later development of our science corresponds entirely with my own understanding."[66]

This essay gave a clear exposition of Planck's (and one may assume, Einstein's) views, both in physics and in philosophy more generally. Thus Planck wrote there:

> The essential point of the positivist theory is that there is no other source of knowledge except the straight and short way of perception through the senses. Positivism always holds strictly to that. Now, the two sentences: (1) *there is a real outer world which exists independently of our act of knowing* and (2) *the real outer world is not directly knowable* form together the cardinal hinge on which the whole structure of physical science turns. And yet there is a certain degree of contradiction between those two sentences. This fact discloses the presence of the irrational, or mystic, element which adheres to physical science as to every other branch of human knowledge. The effect of this is that a science is never in a position completely and exhaustively to solve the problem it has to face. We must accept that as a hard and fast, irrefutable fact, and this fact cannot be removed by a theory which restricts the scope of science at its very start. Therefore, we see the task of science arising before us as an incessant struggle toward a goal which will never be reached, because by its very nature it is unreachable. It is of a metaphysical character, and, as such, is always again and again beyond our achievement.[67]

From then on, Einstein's and Planck's writings on these matters are often almost indistinguishable from each other. Thus, in an essay in honor of Bertrand Russell,[68] Einstein warns that the "fateful 'fear of metaphysics' . . . has come to be a malady of contemporary empiricistic philosophizing." On the other hand, in the numerous letters between the two old friends, Einstein and Besso, each to the very end touchingly and patiently tries to explain his position, and perhaps to change the other's. Thus, on 28 February 1952, Besso once more presents a way of making Mach's views again acceptable to Einstein. The latter, in answering on 20 March 1952, once more responds that the facts cannot lead to a deductive theory and, at most, can set the stage "for intuiting a general principle" as the basis of a deductive theory. A little later, Besso is gently scolded (in

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Einstein's letter of 13 July 1952): "It appears that you do not take the four-dimensionality of reality seriously, but that instead you take the present to be the only reality. What you call 'world' is in physical terminology 'spacelike sections' for which the relativity theory—already the special theory—denies objective reality."

In the end, Einstein came to embrace the view which many, and perhaps he himself, thought earlier he had eliminated from physics in his basic 1905 paper on relativity theory: that there exists an external, objective, physical reality which we may hope to grasp—not directly, empirically, or logically, or with fullest certainty, but at least by an intuitive leap, one that is only guided by experience of the totality of sensible "facts." Events take place in a "real world," of which the space-time world of sensory experience, and even the world of multidimensional continua, are useful conceptions, but no more than that. For a scientist to change his philosophical beliefs so fundamentally is rare, but not unprecedented. Mach himself underwent a dramatic transformation quite early (from Kantian idealism, at about age seventeen or eighteen, according to Mach's autobiographical notes). We have noted that Ostwald changed twice, once to anti-atomism and then back to atomism. And strangely, Planck himself confessed in his 1910 attack on Mach (Ref. 23) that some twenty years earlier, near the beginning of his own career when Planck was in his late twenties (and Mach was in his late forties), he, too, had been counted "one of the decided followers of the Machist philosophy," as indeed is evident in Planck's early essay on the conservation of energy (1887).

In an unpublished fragment apparently intended as an additional critical reply to one of the essays in the collection *Albert Einstein, Philosopher-Scientist* (1949), Einstein returned once more—and quite scathingly—to deal with the opposition. The very words he used showed how complete was the change in his epistemology. Perhaps even without consciously remembering Planck's words in the attack on Mach of 1909 cited earlier—that a basic aim of science is "the complete liberation of the physical world picture from the individuality of the separate intellects"[23]—Einstein refers to a "basic axiom" in his own thinking:

> It is the postulation of a "real world" which so-to-speak liberates the "world" from the thinking and experiencing subject. The extreme positivists think that they can do without it; this seems to me to be an illusion, if they are not willing to renounce thought itself.

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Einstein's final epistemological message was that the world of mere experience must be subjugated by and based in fundamental thought so general that it may be called cosmological in character. To be sure, modern philosophy did not gain thereby a major novel and finished corpus. Physicists the world over generally feel that today one must steer more or less a middle course in the area between, on the one hand, the Machist attachment to empirical data or heuristic proposals as the sole source of theory and, on the other, the aesthetic-mathematical attachment to persuasive internal harmony as the warrant of truth. Moreover, the old dichotomy between rationalism and empiricism is slowly being dissolved in new approaches.

Yet by going in his own philosophical development from one end of this range to the other, and by always stating forthrightly and with eloquence his redefined position, Einstein has helped us to define our own.

REFERENCES

1. Another recently published chapter for this study is the analysis of the probable sources of Einstein's first paper on relativity theory; see *American Scholar*, Vol. 37 (Winter, 1967), pp. 59-79.

2. These documents are mostly on deposit at the Archives of the Estate of Albert Einstein at Princeton; where not otherwise indicated, citations made here are from those documents. In studying and helping to order for scholarly purposes the materials in the Archives, I have benefited from and am grateful for the help received from the Trustees of the Albert Einstein Estate, and particularly from Miss Helen Dukas.

   I thank the Executor of the Estate for permission to quote from the writings of Albert Einstein. I also wish to acknowledge the financial support provided by the Rockefeller Foundation for cataloguing the collection in the Archives at Princeton. The Institute for Advanced Study at Princeton and its director have been most hospitable throughout this continuing work. I am also grateful to M. Vero Besso for permission to quote from the letters of his father, Michelange Besso. All translations here are the author's, unless otherwise indicated.

   Early drafts of portions of this essay have been presented as invited papers at the *Tagung* of *Eranos* in Ascona (August, 1965), at the International Congress for the History of Science in Warsaw (August, 1965), and at the meeting, *Science et Synthèse*, at UNESCO in Paris (December, 1965.

3. F. Herneck, *Forschung und Fortschritte*, Vol. 36 (1964), p. 75. This and the next letter cited have been published by F. Herneck.

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4. The only other known attempt on Einstein's part to obtain an assistantship at that time was a request to Kammerlingh-Onnes (12 April 1901), to which, incidentally, he also seems to have received no response.

5. J. T. Merz, *A History of European Thought in the Nineteenth Century*, Vol. 2 (reprint, Dover Publishing Co., N. Y.; 1965), pp. 184, 199.

6. "Erinnerungen an Albert Einstein," issued by the Patent Office in Berne, about 1965 (n.d., no pagination).

7. Wilhelm Ostwald, *Allgemeine Chemie*, Vol. 2 (2d ed.; 1893), p. 1014.

8. P. A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist* (Evanston, Ill., 1946).

9. *Die Mechanik in ihrer Entwicklung, historisch-kritisch dargestellt* (Leipzig, 1883).

10. In a special supplement on Ernst Mach in the journal *Neue Freie Presse* (Vienna), 12 June 1926.

11. Einstein himself, in a brief and telling analysis, published also in the *Neue Freie Presse* of Vienna on 12 June 1926 (the day of unveiling of a monument to Mach), wrote:

> Ernst Mach's strongest driving force was a philosophical one: the dignity of all scientific concepts and statements rests solely in isolated experiences [*Einzelerlebnisse*] to which the concepts refer. This fundamental proposition exerted mastery over him in all his research, and gave him the strength to examine the traditional fundamental concepts of physics (time, space, inertia) with an independence which at that time was unheard of.

12. Among many evidences of Mach's effectiveness, not the least are his five hundred or more publications (counting all editions—for example, seven editions of his *The Science of Mechanics* in German alone during his lifetime), as well as his large exchange of letters, books, and reprints (of which many important ones "carry the dedication of their authors," to cite the impressive catalogue of Mach's library by Theodor Ackermann, Munich, No. 634 [1959] and No. 636 [1960]). A glimpse of Mach's effect on those near him was furnished by William James, who in 1882 heard Mach give a "beautiful" lecture in Prague. Mach received James "with open arms. . . . Mach came to my hotel and I spent four hours walking and supping with him at his club, an unforgettable conversation. I don't think anyone ever gave me so strong an impression of pure intellectual genius. He apparently has read everything and thought about everything, and has an absolute simplicity of manner and winningness of smile when his face lights up, that are charming." From James's letter, in Gay Wilson Allen, *William James, a Biography* (New York, 1967), p. 249.

The topicality of Mach's early speculations on what is now part of General Relativity Theory is attested by the large number of continuing contributions on the Mach Principle. Beyond that, Mach's influence today

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is still strong in scientific thinking, though few are as explicit and forthright as the distinguished physicist R. H. Dicke of Princeton University in his recent, technical book, *The Theoretical Significance of Experimental Relativity* ([London, 1964] pp. vii-viii): "I was curious to know how many other reasonable theories [in addition to General Relativity] would be supported by the same facts. . . . The reason for limiting the class of theories in this way is to be found in matters of philosophy, not in the observations. Foremost among these considerations was the philosophy of Bishop Berkeley and E. Mach. . . . The philosophy of Berkeley and Mach always lurked in the background and influenced all of my thoughts."

13. Albert Einstein, "Ernst Mach," *Physikalische Zeitschrift*, Vol. 17 (1916), p. 2.

14. Gerald Holton, "On the Origins of the Special Theory of Relativity," *American Journal of Physics*, Vol. 28 (October, 1960), pp. 627-36.

15. Gerald Holton, "Note on the Thematic Analysis of Science: The Case of Poincaré and Relativity," *Melanges Koyré* (Paris, 1964).

16. For evidences that this insistence on prior epistemological analysis of conceptions of space and time are Machist rather than primarily derived from Hume and Kant (who had, however, also been influential), see Einstein's detailed rendition of Mach's critique of Newtonian space and time, in Ref. 13; his discussion of Mach in the *Autobiographical Notes*, pp. 27-29; and in Ref. 1.

17. Albert Einstein, "Zur Elektrodynamik bewegter Körper," *Annalen der Physik*, Vol. 17 (1905), p. 893.

18. See also P. Frank, in P. A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist*, pp. 272-73: "The definition of simultaneity in the special theory of relativity is based on Mach's requirement that every statement in physics has to state relations between observable quantities. . . . There is no doubt that . . . Mach's requirement, the 'positivistic' requirement, was of great heuristic value to Einstein."

19. For example, see Philipp Frank, *Modern Science and Its Philosophy* (New York, 1955), pp. 61-89; V. Kraft, *The Vienna Circle* (New York, 1953); R. von Mises, *Ernst Mach und die Empiristische Wissenschaftsauffassung* (1938; printed as a fascicule of the series *Einheitswissenschaft*).

20. J. Petzoldt, "Gesellschaft für Positivistische Philosophie," reprinted in *Zeitschrift für Positivistische Philosophie*, Vol. 1 (1913), p. 4.

In that same speech, Petzoldt sounded a theme that became widely favored in the positivistic interpretation of the genesis of relativity theory—namely, that the relativity theory was developed in direct response to the puzzle posed by the results of the Michelson experiment:

> Clarity of thinking is inseparable from knowledge of a sufficient number of individual cases for each of the concepts used in investigation. Therefore, the chief requirement of positivistic philosophy: greatest respect for the facts. The newest phase of theoretical physics gives us an exemplary case. There, one does not hesitate, for the sake of a single

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experiment, to undertake a complete reconstruction. The Michelson experiment is the cause and chief support of this reconstruction, namely, the electrodynamic theory of relativity. To do justice to this experiment one has no scruples to submit the foundations of theoretical physics as it has hitherto existed, namely, Newtonian mechanics, to a profound transformation.

In his interesting essay "Das Verhältnis der Machschen Gedankenwelt zur Relativitätstheorie," published as an appendix in the eighth German edition of Mach's *The Science of Mechanics* in the year 1921, Petzoldt faithfully attempts to identify and discuss several Machist aspects of Einstein's relativity theory:
(1) The theory "in the end is based on the recognition of the coincidence of sensations; and therefore it is fully in accord with Mach's worldview, which may be best characterized as a relativistic positivism" (p. 516).
(2) Mach's works "produced the atmosphere without which Einstein's Relativity Theory would not have been possible" (p. 494), and in particular Mach's analysis of the equivalence of rotating reference objects in Newton's bucket experiment prepared for the next step, Einstein's "equivalence of relatively moving coordinate systems" (p. 495).
(3) Mach's principle of economy is said to be marvelously exhibited in Einstein's succinct and simple statements of the two fundamental hypotheses. The postulate of the equivalence of inertial coordinate systems deals with "the simplest case thinkable, which now also serves as a fundamental pillar for the General Theory. And Einstein chose also with relatively greatest simplicity the other basic postulate [constancy of light velocity]. . . . These are the foundations. Everything else is logical consequence" (pp. 497-98).

21. Letter of Besso to Einstein, 16 February 1939. Among many testimonies to the effect of Einstein on positivistic philosophies of science, see P. W. Bridgman, "Einstein's Theory and the Operational Point of View," *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp.

22. Ernst Mach, *History and Root of the Principle of the Conservation of Energy* (Chicago, 1911), translation by P. Jourdain of second edition (1909), p. 95. For a brief analysis of Mach's various expressions of adherence as well as reservations with respect to the principle of relativity, see H. Dingler, *Die Grundlagen der Machschen Philosophie* (Leipzig, 1924), pp. 73-86.

F. Herneck (*Phys. Blätter*, Vol. 17 [1961], p. 276) reports that P. Frank wrote him he had the impression during a discussion with Ernst Mach around 1910 that Mach "was fully in accord with Einstein's special relativity theory, and particularly with its philosophical basis."

23. Republished in M. Planck, *A Survey of Physical Theory* (New York, 1960), p. 24. We shall read later a reaffirmation of this position, in almost exactly the same words, but from another pen.

After Mach's rejoinder (*Scientia*, Vol. 7 [1910], p. 225), Planck wrote a second, much more angry essay, "Zur Machschen Theorie der physikalischen Erkenntnis," *Vierteljahrschrift für wissenschaftliche Philosophie*,

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Vol. 34 (1910), p. 497. He ends as follows: "If the physicist wishes to further his science, he must be a Realist, not an Economist [in the sense of Mach's principle of economy]; that is, in the flux of appearances he must above all search for and unveil that which persists, is not transient, and is independent of human senses."

24. Later Einstein found that this procedure did not work; see *Ideas and Opinions* (New York, 1954), p. 286, and other publications. In a letter of 2 February 1954 to Felix Pirani, Einstein writes: "One shouldn't talk at all any longer of Mach's principle, in my opinion. It arose at a time when one thought that 'ponderable bodies' were the only physical reality and that in a theory all elements that are fully determined by them should be conscientiously avoided. I am quite aware of the fact that for a long time, I, too, was influenced by this fixed idea."

25. *Zeitschrift für Mathematik und physik*, Vol. 62 (1913), pp. 225-61.

26. For a further analysis and the full text of the four letters, see F. Herneck, *Forschungen und Fortschritte*, Vol. 37 (1963), pp. 239-43, and *Wissenschaftliche Zeitschrift der Friedrich-Schiller-Universität Jena*, Vol. 15 (1966), pp. 1-14; and H. Hönl, *Phys. Bl.*, Vol. 16 (1960), p. 571. Many other evidences, direct and indirect, have been published to show Mach's influence on Einstein prior to Mach's death in 1916. For example, recently a document has been found which shows that in 1911 Mach had participated in formulating and signing a manifesto calling for the founding of a society for the positivistic philosophy. Among the signers, together with Mach, we find Joseph Petzoldt, David Hilbert, Felix Klein, George Helm, Sigmund Freud, and Einstein. (See F. Herneck, *Physikalische Blätter*, Vol. 17 [1961], p. 276.)

27. *Physikalische Zeitschrift*, Vol. 11 (1910), p. 605.

28. See Albert Einstein, "Time, Space, and Gravitation" (1948), *Out of My Later Years* (New York, 1950). Einstein makes the distinction between constructive theories and "theories of principle." Einstein cites, as an example of the latter, the relativity theory, and the laws of thermodynamics. Such theories of principle, Einstein says, start with "empirically observed general properties of phenomena."

29. *Bull. Soc. Franc. Phil.*, Vol. 22 (Paris, 1922), p. 111. In his 1913 preface rejecting relativity, Mach expressed himself perhaps more impetuously and irascibly than he may have meant. Some evidence for this possibility is in Mach's letters to J. Petzoldt. On 27 April 1914 Mach wrote: "I have received the copy of the positivistic *Zeitschrift* which contains your article on relativity; I liked it not only because you copiously acknowledge my humble contributions with respect to that theme, but also in general." And on 1 May 1914 Mach writes—rather more incoherently—to Petzoldt: "The enclosed letter of Einstein [a copy of the last of Einstein's four letters, cited above] proves the penetration of positivistic philosophy into physics; you can be glad about it. A year ago, philosophy was altogether sheer nonsense. The details prove it. The paradox of the clock would not have been noticed by Einstein a year ago."

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I thank Mr. John Blackmore for drawing my attention to the Mach-Petzoldt letters, and to Dr. H. Müller for providing copies from the Petzoldt Archive in Berlin.

30. R. S. Shankland, *American Journal of Physics*, Vol. 31 (1963), p. 56.

31. A typical example is a letter of 18 September 1930 to Armin Weiner:

> . . . I did not have a particularly important exchange of letters with Mach. However, Mach did have a considerable influence upon my development through his writings. Whether or to what extent my life's work was influenced thereby is impossible for me to find out. Mach occupied himself in his last years with the relativity theory, and in a preface to a late edition of one of his works even spoke out in rather sharp refusal against the relativity theory. However, there can be no doubt that this was a consequence of a lessening ability to take up [new ideas] owing to his age, for the whole direction of thought of this theory conforms with Mach's, so that Mach quite rightly is considered as a forerunner of general relativity theory. . . .

I thank Colonel Bern Dibner for making a copy of the letter available to me from the Archives of the Burndy Library in Norwalk, Connecticut. Among other hitherto unpublished letters in which Einstein indicated his indebtedness to Mach, we may cite one to A. Lampa, 9 December 1935:

> . . . You speak about Mach as about a man who has gone into oblivion. I cannot believe that this corresponds to the facts since the philosophical orientation of the physicists today is rather close to that of Mach, a circumstance which rests not a little on the influnece of Mach's writings.

Moreover, practically everyone else shared Einstein's explicitly expressed opinion of the debt of relativity theory to Mach; thus H. Reichenbach wrote in 1921 (*Logos*, Vol. 10, p. 331): "Einstein's theory signifies the accomplishment of Mach's program." Even Hugo Dingler agreed: "[Mach's] criticism of the Newtonian conceptions of time and space served as a starting point for the relativity theory. . . . Not only Einstein's work, but even more recent developments, such as Heisenberg's quantum mechanics, have been inspired by the Machian philosophy." *Encyclopedia of the Social Sciences*, Vol. 9 (New York, 1933), p. 653. And H. E. Hering wrote an essay whose title is typical of many others: "Mach als Vorläufer des physikalischen Relativitätsprinzips," *Kölner Universitätszeitung*, Vol. 1 (January 17, 1920), pp. 3-4. I thank Dr. J. Blackmore for a copy of the article.

32. In the special supplement of *Neue Freie Presse* of Vienna on 12 June 1926, Einstein—then already disenchanted for some time with the Machist program—wrote immediately after the portion quoted in Reference 11:

> Philosophers and scientists have often criticized Mach, and correctly so, because he erased the logical independence of the concepts vis-à-vis the "sensations," [and] because he wanted to dissolve the Reality of Being, without whose postulation no physics is possible, in the Reality of Experience. . . .

There are additional resources, both published and unpublished, on the detailed aspects of the relation between Einstein and Mach, which, for lack of space, cannot be summarized here.

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33. For example, by Einstein himself, by Joseph Petzoldt, and by Hugo Dingler (cf. H. Dingler, *Encyclopedia of the Social Sciences,* pp. 84-86). I assign relatively little weight to the possibility that the rift grew out of the difference between Einstein and Mach on atomism. Herneck provides the significant report that according to a letter from P. Frank, Mach was personally influenced by Dingler, whom Mach had praised in the 1912 edition of the *Mechanik* and who was from the beginning an opponent of relativity theory, becoming one of the most "embittered enemies" of Einstein (F. Herneck, *op. cit.* [Ref. 26], p. 14). The copies of letters from Dingler to Mach in the Ernst-Mach-Institute in Freiburg indicate Dingler's intentions; nevertheless, there remains a puzzle about Dingler's role which is worth investigating. It is significant that in his 1921 essay, Petzoldt (Ref. 20) devotes much space to a defense of Einstein's work against Dingler's attacks. See also Joachim Thiele's detailed analysis of Mach's Preface, in *NTM, Schriftenreihe für Geschichte der Naturwissenschaften, Technik und Medizin,* Vol. 2 (Leipzig, 1965), pp. 10-19.

34. "On the Theory of Relativity," *Mein Weltbild* (Amsterdam, 1934); republished in *Ideas and Opinions* (New York, 1954), p. 246. F. Herneck has given the texts of similar discussions on phonographic records by Einstein in 1921 and even in 1924; cf. *Forschungen und Fortschritte,* Vol. 40 (1966), pp. 133-34.

35. Quotations from "On the Method of Theoretical Physics," *Mein Weltbild* (1934), as reprinted in translation in *Ideas and Opinions,* pp. 270-76, except for correction of mistranslation of one line. There are a number of later lectures and essays in which the same point is made. See, for example, the lecture, "Physics and Reality" (1936, reprinted in *Ideas and Opinions*), which states that Mach's theory of knowledge is insufficient on account of the relative closeness between experience and the concepts which it uses; Einstein advocates going beyond this "phenomenological physics" to achieve a theory whose basis may be further removed from direct experience, but which in return has more "unity in the foundations." Or see *Autobiographical Notes,* p. 27: "In the choice of theories in the future," he indicates that the basic concepts and axioms will continue to "distance themselves from what is directly observable."

    Even as Einstein's views developed to encompass the "*erlebbare, beobachtbare*" facts as well as the "*wild-spekulative*" nature of theory, so did those of many of the philosophers of science who also had earlier started from a more strict Machist position. This growing modification of the original position, partly owing to "the growing understanding of the general theory of relativity," has been chronicled by P. Frank, for example, in "Einstein, Mach, and Logical Positivism," in P. A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist.*

36. For a discussion of thematic and phenomenic elements in theory construction, see G. Holton, "The Thematic Imagination in Science," in G. Holton (ed.), *Science and Culture* (Boston, 1965).

37. P. A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist,* p. 53. Em-

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phases added. On pp. 9-11, Einstein describes what may be a possible precursor of this attitude in his study of geometry as a child.

38. W. Kaufmann, "Concerning the Constitution of the Electron," *Annalen der Physik*, Vol. 19 (1906), p. 495. Emphasis in original.

39. *Jahrbuch der Radioaktivitaet und Elektronik*, Vol. 4 (1907), p. 28. Shortly after Kaufmann's article appeared, M. Planck (in *Physikalische Zeitschrift*, Vol. 7 [1906], pp. 753-61) took it on himself publicly to defend Einstein's work in an analysis of Kaufmann's claim. He concluded that Kaufmann's data did not have sufficient precision for his claim. Incidentally, Planck tried to coin the term for the new theory that had not yet been named: "Relativtheorie."

40. It should be remembered that Poincaré, with a much longer investment in attempts to fashion a theory of relativity, was quite ready to give in to the experimental "evidence." See Ref. 15.

41. Carl Seelig, *Albert Einstein* (Zürich, 1960), p. 195.

42. Albert Einstein, "Notes on the Origin of the General Theory of Relativity," *Mein Weltbild* (Amsterdam, 1934), reprinted in translation in *Ideas and Opinions* (New York, 1954), pp. 285-90.

43. *"Da könnt' mir halt der liebe Gott leid tun, die Theorie stimmt doch."* This semi-serious remark of a person who was anything but sacrilegious indeed illuminates the whole style of a significant group of new physicists. P. A. M. Dirac, in *Scientific American*, Vol. 208 (May 1963), pp. 47-48, speaks about this, with special attention to the work of Schrödinger, a spirit close to that of his friend, Einstein, despite the ambivalence of the latter to the advances in quantum physics. We can do no better than quote *in extenso* from Dirac's account [pp. 46-47]:

> Schrödinger worked from a more mathematical point of view, trying to find a beautiful theory for describing atomic events, and was helped by deBroglie's ideas of waves associated with particles. He was able to extend deBroglie's ideas and to get a very beautiful equation, known as Schrödinger's wave equation, for describing atomic processes. Schrödinger got this equation by pure thought, looking for some beautiful generalization of deBroglie's ideas, and not by keeping close to the experimental development of the subject in the way Heisenberg did.
>
> I might tell you the story I heard from Schrödinger of how, when he first got the idea for this equation, he immediately applied it to the behavior of the electron in the hydrogen atom, and then he got results that did not agree with experiment. The disagreement arose because at that time it was not known that the electron has a spin. That, of course, was a great disappointment to Schrödinger, and it caused him to abandon the work for some months. Then he noticed that if he applied the theory in a more approximate way, not taking into account the refinements required by relativity, to this rough approximation his work was in agreement with observation. He published his first

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paper with only this rough approximation, and in this way Schrödinger's wave equation was presented to the world. Afterward, of course, when people found out how to take into account correctly the spin of the electron the discrepancy between the results of applying Schrödinger's relativistic equation and the experiments was completely cleared up.

I think there is a moral to this story, namely, that it is more important to have beauty in one's equations than to have them fit experiment. If Schrödinger had been more confident of his work, he could have published it some months earlier, and he could have published a more accurate equation. That equation is now known as the Klein-Gordon equation, although it was really discovered by Schrödinger, and in fact was discovered by Schrödinger before he discovered his nonrelativistic treatment of the hydrogen atom. It seems that if one is working from the point of view of getting beauty in one's equations, and if one has really a sound insight, one is on a sure line of progress. If there is not complete agreement between the results of one's work and experiment, one should not allow oneself to be too discouraged, because the discrepancy may well be due to minor features that are not properly taken into account and that will get cleared up with further developments of the theory. That is how quantum mechanics was discovered . . . .

44. Published several times—for example, by Teubner, Leipzig, 1909.

45. F. Herneck, *Physikalische Blätter*, Vol. 15 (1959), p. 565. P. Frank's remark is reported by F. Herneck, in *Ernst Mach*, ed. W. F. Merzkirch (Freiburg, 1967), p. 50.

46. *Zeitschrift für Physikalische Chemie* (1910), pp. 466-95.

47. *Mach's Empirio-Pragmatism in Physical Science* (Thesis, Columbia University, 1937).

48. Ernst Mach, *Scientia*, Vol. 7 (1910), p. 225.

49. *Space and Geometry* (1906), p. 138. Mach's attempts to speculate on the use of n-dimensional spaces for representing the configuration of such "mere things of thought"—the derogatory phrase also applied to absolute space and absolute motion in Newton—are found in his first major book, *Conservation of Energy* (first edition, 1872).

50. Cf. J. Petzoldt, "Verbietet die Relativitätstheorie Raum und Zeit als etwas Wirkliches zu denken?," *Verhandlungen der Deutschen Physikalischen Gesellschaft*, Nos. 21-24 (1918), pp. 189-201. Here, and again in his 1921 essay (Ref. 20), Petzoldt tries to protect Einstein from the charge—for example, that by Sommerfeld—that space and time no longer "are to be thought of as real."

51. Ernst Mach, *Conservation of Energy*, p. 54.

52. *Op. cit.* Ref. 29; also reported in "Einstein and the Philosophies of Kant and Mach," *Nature*, Vol. 112 (August, 1923), p. 253.

53. That Einstein did not so misunderstand Newton can be illustrated, for example, in a comment reported by C. B. Weinberg: "Dr. Einstein further

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maintained that Mach, as well as Newton, tacitly employs hypotheses—not recognizing their non-empirical foundations." (Weinberg, *op. cit.*, p. 55.) For an example of Mach's tacit presuppositions, see H. Dingler, *Encyclopedia of the Social Sciences*, pp. 69-71. Dingler also analyzed some of the non-empirical foundations of relativity theory in *Kritische Bemerkungen zu den Grundlagen der Relativitätstheorie* (Leipzig, 1921).

54. Cited by R. S. Cohen in his very useful essay in P. Schilpp (ed.), *The Philosophy of Rudolf Carnap* (LaSalle, Ill., 1963), p. 109. I am also grateful to Professor Cohen for a critique of parts of this paper in earlier form.

55. Albert Einstein, "Notes on the Origin of the General Relativity Theory," pp. 288-89.

56. Ref. 25, pp. 230-31.

57. I am not touching in this essay on the effect of quantum mechanics on Einstein's epistemological development; the chief reason is that while from his "heuristic" announcement of the value of a quantum theory in 1905 Einstein remained consistently skeptical about the "reality" of the quantum theory of radiation, this opinion only added to the growing realism stemming from his work on general relativity theory. In the end, he reached the same position in quantum physics as in relativity; cf. his letter of 7 September 1944 to Max Born: "In our scientific expectations we have become antipodes. You believe in the dice-playing God, and I in perfect rules of law in a world of something objectively existing, which I try to catch in a wildly speculative way." (Reported by Max Born, *Universitas*, Vol. 8 [1965], p. 33.)

58. Max Born, "Physics and Relativity," *Physics in My Generation* (London, 1956), p. 205.

59. *Orell Füssli Verlag* (Zürich and Leipzig, 1929), pp. 126-32. I am grateful to Professor C. Lanczos and Professor John Wheeler for pointing out this reference to me.

60. H. Dingler, *Die Grundlagen der Machschen Philosophie*, p. 98.

61. *The Philosophy of Physics* (New York, 1936), pp. 122, 125.

62. In "Religion and Science," written for *The New York Times Magazine*, 9 November 1930; cf. *Mein Weltbild*, p. 39, and *Cosmic Religion* (New York, 1931), p. 48.

Possible reasons for Einstein's growing interest in these matters, partly related to the worsening political situation at the time, are discussed in P. Frank, *Einstein, His Life and Times*. It is noteworthy that while Einstein was quite unconcerned with religious matters during the period of his early scientific publications, he gradually returned later to a position closer to that at a very early age, when he reported he had felt a "deep religiosity. . . . It is quite clear to me that the religious paradise of youth . . . was a first attempt to free myself from the chains of the 'merely personal.'" *Autobiographical Notes*, p. 5. For a discussion, see G. Holton, *The Graduate Journal*, Vol. 7 (Spring 1967), pp. 417-20.

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63. P. Frank, *Einstein, His Life and Times* (New York, 1947), p. 215. Einstein's change of mind was, of course, not acceptable to a considerable circle of previously sympathetic scientists and philosophers. See for example P. W. Bridgman, "Einstein's Theory and the Operational Point of View," in P. A. Schilpp (ed.), *Albert Einstein: Philosopher-Scientist.*

64. Originally entitled "Motiv des Forschens" (in *Zu Max Planck,* Planck's 60. *Geburtstag* [Karlsruhe, Müller, 1918]), the talk had a fate not untypical of many of Einstein's essays. It was reprinted without date or source, under the title "Prinzipien der Forschung," in *Mein Weltbild,* and again, in perhaps unauthorized extension and translation by James Murphy, as a preface to M. Planck, *Where Is Science Going?* (London, 1933). In an earlier appreciation of Planck in 1913, Einstein had written only very briefly about Planck's epistemology, merely lauding Planck's essay of 1896 against energetics, and not mentioning Mach.

65. *International Forum,* Vol. 1, Nos. 1-2 (1931).

66. Einstein sent his introduction to the editor of the journal on 17 April 1931, but it appears to have come too late for inclusion.

67. Max Planck, "Positivism and External Reality," *The International Forum,* Vol. 1 (Berlin 1931), Nos. 1-2, pp. 15-16. Emphasis in original.

68. In *The Philosophy of Bertrand Russell,* ed. P. Schilpp (Evanston, 1944), p. 289.

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Notes on Contributors

JOSEPH AMBROSE BANKS, born in 1920, is senior research lecturer in industrial sociology in the department of social science at the University of Liverpool. Mr. Banks is the author of *Prosperity and Parenthood* (London, 1954) and *Industrial Participation* (Liverpool, 1963). With his wife, Olive Banks, he co-authored *Feminism and Family Planning* (Liverpool, 1964).

BERNARD H. SLICHER VAN BATH, born in 1910, is professor of agrarian history at Wageningen University in the Netherlands. For the 1967-68 academic year, he has been visiting research professor at the University of Chicago. He is the author of *The Agrarian History of Western Europe A. D. 500-1850* (London, 1963) and *Yield Ratios 810-1920* (Wageningen, 1963).

JEAN-NOEL BIRABEN, born in 1928, is assistant director of the Institut National d'Etudes Démographiques in Paris. He has published numerous articles in *Population* since 1957.

PAUL DEMENY, born in 1932, is associate professor of economics at the University of Michigan. He is the co-author of *Regional Model Life Tables and Stable Populations* (Princeton, 1965), *Methods of Estimating Basic Demographic Measures from Incomplete Data* (New York, 1967), and *The Demography of Tropical Africa* (Princeton, 1967).

DAVID V. GLASS, born in 1911, is Martin White Professor of Sociology at the University of London (London School of Economics). Mr. Glass is the author of *Population Policies and Movements in Europe* (Oxford, 1940; reprinted in 1967), *Introduction to Malthus* (London, 1954), *Social Mobility in Britain* (London, 1954); he is the co-author of *The Trend and Pattern of Fertility in Britain* (London, 1955) and co-editor of *Population in History* (London, 1965).

PIERRE GOUBERT, born in 1915, is professor of modern history in the Faculté des Lettres et Sciences Humaines of the University of Paris-Nanterre; he is also at the Ecole Pratique des Hautes Etudes at the Sorbonne. Mr. Goubert is the author of *Beauvais et le Beauvaisis de 1600 à 1730* (Paris, 1960), *1789, Les Français ont la parole* (Paris, 1964), and *Louis XIV et vingt millions de Français* (Paris, 1966).

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SUSAN B. HANLEY, born in 1939, is a graduate student in history at Yale University. Miss Hanley has translated a number of articles on Japanese agriculture which have been published in English in Japan.

DAVID M. HEER, born in 1930, is assistant professor of biostatistics and demography at the School of Public Health of Harvard University. He is the author of *After Nuclear Attack: A Demographic Inquiry* (New York, 1965), *Society and Population* (Englewood Cliffs, N. J., 1968), and editor of *Readings on Population* (Englewood Cliffs, N. J., 1968).

LOUIS HENRY, born in 1911, is director of the Institut National d'Etudes Démographiques. Mr. Henry's most recent publications include *Perspectives démographiques* (Paris, 1964); *Manuel de démographie historique* (Geneva, 1967); *Nouveau manuel de dépouillement et d'exploitation de l'état civil ancien* (Paris, 1965), with Michel Fleury; and *La population de Crulai, paroisse normande* (Paris, 1958), with Etienne Gautier.

THOMAS HENRY HOLLINGSWORTH, born in 1932, is lecturer in social statistics at the University of Glasgow. Mr. Hollingsworth is the author of *The Demography of the British Peerage*, Supplement to *Population Studies*, Vol. 18, No. 2 (Nov., 1964).

GERALD HOLTON, born in 1922, is professor of physics and a member of the history of science department at Harvard University. From 1957 to 1963, he was editor of the American Academy of Arts and Sciences and, until 1961, editor of *Dædalus*. His publications include *Introduction to Concepts and Theories in Physical Science, Experimental Physics*, and (as editor) *Science and the Modern Mind*.

DAVID LANDES, born in 1924, is professor of history at Harvard University, where he is also director of the Center for Middle Eastern Studies. Mr. Landes is the author of *Bankers and Pashas: International Finance and Economic Imperialism* (London, 1958) and *The Unbound Prometheus: Technological Change and Industrial Development in Western Europe Since 1750* (Cambridge, 1968).

MASSIMO LIVI-BACCI, born in 1936, is professor of demography at the University of Florence and research staff member of the Office of Population Research at Princeton University. Mr. Livi-Bacci is the author of *L'immigrazione e l'assimilazione degli Italiani in America* (Milan, 1961) and *I Fattori demografici dello sviluppo economico Italiano* (Rome, 1965).

JOHN T. NOONAN, JR., born in 1926, is professor of law at the University of California, Berkeley. Mr. Noonan is the author of *The Scholastic Analysis of Usury* (Cambridge, 1957) and *Contraception* (Cambridge, 1965). Mr. Noonan was a consultant to the Papal Commission on Problems of the Family, Population, and Natality, 1965-66, and is editor of *The Natural Law Forum*.

ROGER REVELLE, born in 1909, is Richard Saltonstall Professor of Popu-

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lation Policy and director of the Center for Population Studies at Harvard University. Mr. Revelle was director of the Scripps Institution of Oceanography from 1951 to 1964. He has been active in UNESCO for the last ten years, serving as vice-chairman of its U.S. National Commission. Mr. Revelle is at present chairman of the U.S. National Committee on the International Biological Program.

JOSEPH J. SPENGLER, born in 1902, is James B. Duke Professor of Economics at Duke University. Mr. Spengler is the author of *France Faces Depopulation* (Durham, 1938) and *French Predecessors to Malthus* (1942); he is also co-editor of *Tradition, Values, and Socio-Economic Development* (Durham, 1961), *Administration and Economic Development in India* (Durham, 1963), *Population Theory and Policy* (Glencoe, 1956), and *Demographic Analysis* (Glencoe, 1956).

ETIENNE VAN DE WALLE, born in 1932, is research demographer in the Office of Population Research at Princeton University. He is the co-author of *The Demography of Tropical Africa* (Princeton, 1968).

EDWARD ANTHONY WRIGLEY, born in 1931, is fellow and bursar of Peterhouse, Cambridge University. Mr. Wrigley was the co-founder of the Cambridge Group for the History of Population and Social Structure. He is the author of *Industrial Growth and Population Change* (Cambridge, 1961) and the editor of *An Introduction to English Historical Demography* (London, 1966).

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